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THE

BOSTON JOURNAL

OF

Philosophy and the Arts,

INTENDED TO EXHIBIT A VIEW OF THE

PROGRESS OF DISCOVERY

IN

NATURAL PHILOSOPHY, MECHANICS, CHEMISTRY, GEOLOGY AND MINERALOGY,
NATURAL HISTORY, COMPARATIVE ANATOMY AND PHYSIOLOGY,
GEOGRAPHY, STATISTICS,

AND THE FINE AND USEFUL ARTS.

CONDUCTED BY

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ADVERTISEMENT.

The present number completes the third volume of this journal, and with it the editors take their leave of the public. From the commencement of their work, the editors have never expected an extensive subscription. The pursuits of this busy and thriving community do not lead it to a very lively interest in the subjects to which this publication has been devoted, and although the subscription has been sufficient to defray the expense of printing, the labour of conducting the work has been without reward.

Under these circumstances, the editors cannot any longer devote the time necessary to make selections from foreign works, and to furnish such original matter as shall make their journal satisfactory to themselves or worthy of their subscribers. Far from intending to express any thing like disapprobation or complaint that a larger portion of the public have not a taste for the kind of reading which has been presented in this journal, the editors have made the preceding statement merely to show their patrons, openly, the cause of the discontinuance of their work. The editors feel themselves warranted in attributing their want of patronage to the peculiarity of the public taste, in regard to works of this kind, generally, rather than to the individual character of the papers which they have published, from the fact, that the selected articles, which comprise the greater part of this journal, were written by men most celebrated for genius and attainments in Europe, and on subjects which have occupied a great share of the attention of the scientific world during the progress of this publication.

Boston, December, 1826.

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PHILOSOPHY AND THE ARTS.

ART. I.—*Biographical Account of M. de Fourcroy.* By THOMAS THOMSON, M. D. F. R. S. [*Ann. Philos.*]

LITERARY men may be divided into three classes. Some make a great figure during their life time; but death erases their names from the annals of science, and they sink into the grave and obscurity at once. Such were Dr Mead and Sir John Hill. Some are little known during their life time, and spend their days in obscurity and penury; but when death has once closed the scene, their reputation rises untarnished by envy, and unsullied by emulation, and flows on like a mighty river, the broader, and deeper, and greater, the farther it advances. Such, in some respects, were Kepler and Scheele. Some are so unfortunate, through imprudence, or a perverse train of circumstances, neither to acquire reputation during their lives, nor after their death; while their more fortunate contemporaries, with less labour, and less merit, gather all the laurels which they had earned. It would be invidious to mention the names of any who unfortunately belong to this class; but they will readily occur to every one acquainted with the history of science. Every tyro in algebra is familiar with Cardan's rules for the solution of cubic equations, while the name of the real discoverer of these rules is scarcely known, except to mathematical antiquaries. M. de Fourcroy, the subject of this article, made so conspicuous a figure during his life time, that it would by no means surprise us, if he should finally take his place among that class of literary men, whom we characterised in the first place; not that he wanted merit; for it is not so much merit, as a regard to distributive

justice, which leads to the classification. Who will be hardy enough to affirm that Churchill wanted merit as a poet? During his short and rapid literary career he appeared to wield the thunderbolts in his hand, and was an object of dread and adoration, like a kind of Divinity. But where is his reputation now? It has sunk, since his death, as much below the true level, as it rose above it during his life time; and this we believe will always be the case. Mankind will atone for the excessive adulation which they pay to a man during his lifetime, by a corresponding negligence after his death.

Antoine François de Fourcroy, Compte of the French empire, Counsellor of State, Commander of the Legion of Honour, Member of the Institute, and of most scientific societies in Europe, Professor of Chemistry at the Museum of Natural History, Professor of the Faculty of Medicine at Paris, and teacher in the Polytechnic School, was born at Paris, on the 15th of June, 1755, and was the son of Jean Michel de Fourcroy and of Jeanne Laugier.

His family had long resided in the capital, and several of his ancestors had distinguished themselves at the bar. One of them, during the reign of Charles IX. was honored with the epithet of *fori decus*.

Antoine François de Fourcroy sprung from a branch of the family that had gradually sunk into poverty. His father exercised in Paris the trade of an apothecary, in consequence of a charge which he held in the house of the Duke of Orleans. The Corporation of Apothecaries having obtained the general suppression of all such charges, M. de Fourcroy, the father, was obliged to renounce his mode of livelihood; and his son grew up in the midst of the poverty produced by the monopoly of the privileged bodies in Paris. He felt this situation the more keenly, because he possessed from nature, an extreme sensibility of temper. When he lost his mother, at the age of seven years, he attempted to throw himself into her grave. The care of an elder sister preserved him with difficulty, till he reached the age at which it was usual to be sent to the college. Here he was unlucky enough to meet with a brutal master, who conceived an aversion to him, and treated him with cruelty. The consequence was a dislike to study; and he quitted the college at the age of 14, somewhat less informed than when he went to it.

His poverty now was such, that he was under the necessity of endeavouring to support himself by commencing writing master. He had even some thoughts of going upon the stage,

but was prevented by the hisses bestowed upon a friend of his, who had unadvisedly entered upon that perilous career, and was treated in consequence, without mercy by the audience. While uncertain what plan to follow, the advice of Viq. d'Azyr induced him to commence the study of medicine.

This great anatomist was an acquaintance of M. de Fourcroy, the father. Struck with the appearance of his son, and the courage with which he struggled with his bad fortune, he conceived an affection for him, and promised to direct his studies, and even to assist him during their progress. The study of medicine to a man in his situation was by no means an easy task. He was obliged to lodge in a garret, so low in the roof, that he could only stand upright in the centre of the room. Beside him lodged a water-carrier, with a family of twelve children. Fourcroy acted as physician to this numerous family; and in recompense was always supplied with abundance of water. He contrived to support himself by giving lessons to other students, by facilitating the researches of richer writers, and by some translations which he sold to a bookseller. For these he was only half paid; but the conscientious bookseller offered, thirty years afterwards, to make up the deficiency, when his creditor was become Director General of Public Instruction.

Fourcroy studied with so much zeal and ardour that he soon became well acquainted with the subject of medicine. But this was not sufficient. It was necessary to get a Doctor's degree; and all the expenses, at that time, amounted to £250 sterling. An old physician, Dr Diest, had left funds to the faculty to give a gratuitous degree and license, one every two years, to the poor student, who should best deserve them. Fourcroy was the most conspicuous student at that time in Paris. He would therefore have reaped the benefit of this benevolent institution had it not been for the unlucky situation in which he was placed. There happened to exist a quarrel between the faculty charged with the education of medical men and the granting of degrees, and a society, recently established by government for the improvement of the medical art. This dispute had been carried to a great length, and had attracted the attention of all the frivolous and idle inhabitants of Paris. Viq. d'Azyr was secretary to the society, and of course one of its most active champions, and was in consequence particularly obnoxious to the faculty of medicine at Paris. Fourcroy was unluckily the acknowledged protegée of this eminent anatomist. This was suffi-

cient to induce the faculty of medicine to refuse him a gratuitous degree. He would have been excluded in consequence from entering upon the career of a practitioner, had not the society, enraged at this treatment, and influenced by a violent party spirit, formed a subscription, and contributed the necessary expenses.

It was no longer possible to refuse M. de Fourcroy the degree of Doctor, when he was thus enabled to pay for it. But above the simple degree of Doctor, there was a higher one, entitled Docteur Regent, which depended entirely upon the votes of the faculty. It was unanimously refused to M. de Fourcroy. This refusal put it out of his power afterwards to commence teacher in the medical school and gave the medical faculty the melancholy satisfaction of not being able to enrol among their number the most celebrated professor in Paris. This violent and unjust conduct of the faculty of medicine made a deep impression in the mind of Fourcroy, and contributed not a little to the subsequent downfall of that powerful body.

Fourcroy being thus entitled to practise in Paris, his success depended entirely upon the reputation which he could contrive to establish. For this purpose he devoted himself to the sciences connected with medicine, as the shortest and most certain road by which he could reach his object. His first writings showed no predilection for any particular branch of science. He wrote upon chemistry, anatomy, and natural history. He published an abridgment of the History of Insects, and a Description of the Bursæ Mucosæ of the Tendons. This last piece seems to have given him the greatest celebrity; for in 1785 he was admitted, in consequence of it, into the Academy of Sciences, as an anatomist; but the reputation of Bucquet, which at that time was very high, gradually directed his particular attention to chemistry, and he retained this predilection during the rest of his life.

Bucquet was at that time professor of chemistry in the medical school of Paris, and was then greatly celebrated and followed, on account of his eloquence and the elegance of his language. Fourcroy became in the first place his pupil, and soon after his particular friend. One day, when an unforeseen disease prevented him from lecturing as usual, he entreated M. de Fourcroy to supply his place. The young philosopher at first declined, and alleged his total ignorance of the method of addressing a popular audience. But, overcome by the persuasions of Bucquet, he at last consented;

and in this his first essay, he spoke two hours without disorder or hesitation, and acquitted himself to the satisfaction of his whole audience. Bucquet soon after substituted him in his place, and it was in his laboratory and in his class room that he first made himself acquainted with chemistry. He was enabled at the death of Bucquet, in consequence of an advantageous marriage which he had made, to purchase the apparatus and cabinet of his master; and although the Faculty of Medicine would not allow him to succeed to the chair of Bucquet, they could not prevent him from succeeding to his reputation.

There was a kind of college established in the King's Garden, which was at that time under the superintendance of Buffon, and Macquer was the professor of chemistry in this institution. On the death of this chemist, in 1784, Lavoisier stood candidate for the chair. But Buffon received more than a hundred letters in favour of Fourcroy; and the voice of the public was so loud in his favour, that he was appointed to the situation, in spite of the high reputation of his antagonist, and the superior interest that might be supposed to result from his fortune and his situation.

Fourcroy continued professor at the Jardin des Plantes, during the remainder of his life, which lasted twenty-five years; and such was his eloquence, or so well was it fitted to the taste of the French nation, that his celebrity as a lecturer continued always upon the increase; so great also were the crowds, both of men and women, that flocked to hear him, that it was twice necessary to enlarge the lecture-room. I had myself an opportunity of hearing him lecture two or three times, and must acknowledge that I found it difficult to account for the celebrity which he enjoyed. His style was precisely similar to that of his books, flowing and harmonious, but very diffuse, and destitute of precision; and his manner was that of a petit maitre, mixed with a good deal of pomposity, and an affectation of profundity. There must be something, however, in such a manner, capable of attracting the generality of mankind; for I know a professor, who possesses as much of it as is consistent with the British character, and who is far inferior to Fourcroy as a man of science; who nevertheless, enjoys within his own sphere, nearly the same degree of popularity that Fourcroy did in his.

We must now notice the political career which Fourcroy ran, during the progress of the revolution. In a country

where political changes were going on with so much rapidity, and where every description of men were successively had recourse to, it was not possible that a professor so much admired for his eloquence, could escape observation. Accordingly he was elected a member of the National Convention in the autumn of 1793. The National Convention, and France herself, were at that time in a state of abject slavery; and so sanguinary was the tyrant who ruled over that unhappy country, that it was almost equally dangerous for the members of the Convention to remain silent, or to take an active part in the business of that assembly. Fourcroy, notwithstanding his reputation for eloquence, and the love of eclat which appears all along to have been his domineering passion, had good sense enough to resist the temptation, and never opened his mouth in the Convention till after the death of Robespierre. This is more to be wondered at, and is a greater proof of prudence, as it is well known that he took a keen part in favour of the revolution, and that he was a determined enemy to the old order of things, from which he had suffered so severely at his entrance into life.

At this period he had influence enough to save the life of some men of merit. Among others, of Darcet, who did not know the obligation he lay under to him, till long after. At last his own life was threatened, and his influence, of course, utterly annihilated.

During this unfortunate and disgraceful period, several of the most eminent literary characters of France were destroyed; among others, Lavoisier; and Fourcroy has been accused of contributing to the death of this illustrious philosopher, his former rival, and his master in chemistry. How far such an accusation is deserving of credit, I, for my part, have no means for determining; but Cuvier, who was upon the spot, and in a situation which enabled him to investigate its truth or falsehood, acquits Fourcroy entirely of the charge, and declares that it was urged against him, merely out of envy at his subsequent elevation. "If in the rigorous researches which we have made," says Cuvier in his Eloge of Fourcroy, "we had found the smallest proof of an atrocity so horrible, no human power could have induced us to sully our mouths with his Eloge, or to have pronounced it within the walls of this temple, which ought to be no less sacred to honor than to genius."

Fourcroy began to acquire influence only after the ninth thermidor, when the nation was wearied with destruction, and

when efforts were making to restore those monuments of science, and those public institutions for education, which, during the wantonness and folly of the revolution, had been overturned and destroyed. Fourcroy was particularly active in this renovation, and it was to him chiefly, that almost all the schools established in France for the education of youth, are to be ascribed. The convention had destroyed all the colleges, and universities, and academies, throughout France. The effects of this ridiculous abolition, soon became visible. The army stood in need of surgeons and physicians, and there were none educated to supply the vacant places. Three new schools were founded for educating medical men. They were nobly endowed, and still continue connected with the University of Paris. The term, Schools of Medicine, was proscribed as too aristocratical. They were distinguished by the ridiculous appellation of Schools of Health. The Polytechnic school was next instituted, as a kind of preparation for the exercise of the military profession, where young men could be instructed in mathematics and natural philosophy, to make them fit for entering the schools of the artillery, of engineers, and of the marine. The central school was another institution for which France is indebted to the efforts of Fourcroy. The idea was good, though it has been very imperfectly put in execution. It was to establish a kind of university in every department, for which the young men were to be prepared by means of a sufficient number of inferior schools scattered through the department. But these inferior schools have never been properly established or endowed; and even the central schools themselves have never been supplied with proper masters. Indeed it would have been impossible to have furnished such a number of masters at once. On that account, an institution was established at Paris, under the name of Normal School, for the express purpose of educating a sufficient number of masters to supply the different central schools.

Fourcroy, either as a member of the Convention, or of the Council of Ancients, took an active part in all these institutions, both as far as regarded the plan and the establishment. He was equally concerned in the establishment of the Institute, and of the Museum d'Histoire Naturelle. This last was endowed with the utmost liberality, and Fourcroy was one of the first professors; as he was, also, in the school of medicine, and the Polytechnic school. He was equally concerned in the restoration of the University, which constitutes the

most splendid part of Buonaparte's reign, and the part which will be longest remembered with gratitude and applause.

The violent exertions which M. de Fourcroy made in the numerous situations which he filled, and the prodigious activity which he displayed, gradually undermined his constitution. He himself was sensible of his approaching death, and announced it to his friends as an event which would speedily take place. On the 16th of December, 1809, after signing some despatches, he suddenly cried out, "Je suis mort," and dropt down lifeless on the ground.

He was twice married; first to Mademoiselle Bettinger, by whom he had two children; a son, an officer in the artillery, who inherits his title; and a daughter, Madame Foucaud. He was married a second time to Madame Belleville, the widow of Vailly, by whom he had no family. He left but little fortune behind him; and two maiden sisters who lived with him depended for their support upon his friend, M. Vauquelin.

The character of M. Fourcroy is sufficiently obvious. It was exactly fitted to the country in which he lived, and the revolutionary government, in the midst of which he was destined to finish his career. Vanity was his ruling passion, and the master-spring of all his actions. It was the source of all the happiness and of all the misery of his life; for every attack, from what quarter soever it proceeded, was felt by him with equal acuteness. The sneer of the most ignorant pretender, or the most obscure paper, affected him just as much as if it had proceeded from the most profound philosopher. It is needless to observe, after this, how much he must have suffered from the various parties into which the French chemists divided themselves; all of which were more or less hostile to him, excepting the one which he himself headed. His occupations were too numerous, and his elocution too ready, to put it in his power either to make profound discoveries, or to compose treatises of great depth or originality. The changes which took place in the science of chemistry were brought about by others, who were placed in a different situation, and endowed with different talents; but no man contributed so much as Fourcroy to the popularity of the Lavoisierian opinions, and the rapidity with which they were propagated over France and most countries in Europe. His eloquence drew crowds to hear him, and persuaded his audience to embrace his opinions.

He must have possessed an uncommon facility in writing, for his literary labours are exceedingly numerous. Besides those essays which have been already noticed, he published five editions of his System of Chemistry, each of them gradually increasing in size and value; the first edition being in two volumes, and the fifth in ten. This last edition he wrote in sixteen months. It contains a vast quantity of valuable matter, and contributed considerably to the general diffusion of chemical knowledge. Its fault is the diffuseness of the style, and the want of correct references. The readers of Fourcroy's system would suppose that all the discoveries in chemistry have been made by the French, and that other nations have contributed comparatively little to the stock of chemical knowledge; whereas in reality, the very opposite is the truth. A much greater number of important chemical discoveries have been made in Britain than in France; and the British chemists have contributed prodigiously to the raising of that beautiful fabric which we at present admire.

Perhaps the best of all Fourcroy's productions is his Philosophy of Chemistry, which is remarkable for its conciseness, its perspicuity, and the neatness of its arrangement.

Besides these works, and the periodical work called Le Medicin Eclairé, of which he was the editor, there are above 160 papers on chemical subjects, with his name attached to them as the author, which appeared in the Memoirs of the Academy of the Institute, in the Annales de Chimie, or the Annales de Museum d'Histoire Naturelle, of which last work he was the original projector. As in most of these papers the name of Vauquelin is associated with his own, as the author; and as during the publication of those which appeared with his own name alone, Vauquelin was the operator in his laboratory, it is not possible to determine what part of the experiments were made by Fourcroy, and what by Vauquelin. I have been told, by a gentleman who had a good opportunity of getting information on the subject, that almost all the experiments were made by Vauquelin, but that all the papers were written by Fourcroy himself. The discoveries contained in these numerous dissertations, are of considerable importance, and relate chiefly to animal and vegetable chemistry. At the same time, it must be allowed, that Fourcroy and Vauquelin often fall into mistakes difficult to be accounted for; and that they do not, in every case, do justice to their predecessors or contemporaries, who had been occupied with the same investigations. A complete enumeration of

these dissertations would probably be considered as too tedious. I shall therefore satisfy myself with pointing out some of his most important chemical discoveries and observations.

1. He repeated the curious experiments of Berthollet upon the evolution of azotic gas from animal substances. This dissertation contains very little new, and is remarkable for some striking mistakes; as for example, that azotic gas has the property of giving a green colour to vegetable blues. If such an observation was ever actually made, he must have been deceived by a portion of ammonia mixed with the azotic gas. He announced, soon after, that the air contained in the swimming bladder of the carp is azotic gas. This paper contains several absurd observations on the method of procuring azotic gas; as for example, that the black oxide of manganese gives out pure azote, if exposed to a heat below redness.

2. He analysed a green coloured mineral from Auvergne, which he found a mixture of arseniate of lead and phosphate of lead.

3. He affirmed that ammonia is decomposed by the oxides of manganese, mercury, and iron; and that these oxides, at the same time, lose either the whole or a portion of their oxygen.

4. He ascertained that the most common constituent of biliary calculi is a substance very similar in its properties to spermaceti. This substance, in consequence of a subsequent discovery which he made, during the removal of dead bodies from the burial ground of the Innocents at Paris, namely, that these bodies were converted into a fatty matter, got the name of adipocire.

5. He found that vegetable juices frequently contain a substance which coagulates when the juice is exposed to a gentle heat. This substance he considered as albumen; but Proust afterwards showed that it was, in reality, a species of gluten, and quite different in its properties from albumen.

6. He ascertained the properties of several triple salts, which magnesia, and ammonia, and an acid are capable of forming; and explained, by this discovery, the reason why magnesia is not precipitated completely from its solutions by ammonia. This paper, which appeared in the fourth volume of the Annales de Chimie, I consider as one of the best ever published by Fourcroy.

7. He published a very elaborate analysis of the quinquina, a species of bark from St Domingo, which was considered at the time as a model for vegetable analysis. It bears

the exact characters of the peculiar method followed by Vauquelin. I should suppose therefore, though nothing is said on the subject, that the experiments were contrived and executed by that eminent chemist.

8. His dissertation on the sulphate of mercury, though imperfect, contains some good observations, and facilitated the knowledge of metallic salts, which was at that time very imperfect, but has been greatly improved since. The same remark applies to his subsequent memoirs on the action of ammonia, on the sulphate, nitrate, and muriate of mercury. These papers contain some mistaken opinions, though the formation of the triple salts, which constitutes the basis of his opinion, be correct.

9. His experiments on the brain contain several valuable facts, and his opinion approaches to accuracy. The subject has been recently resumed by Vauquelin, who has published a curious dissertation on it.

10. The analysis of tears, and the mucus of the nose, by Fourcroy and Vauquelin, is valuable; though it contains some mistakes, from the too hasty application of an erroneous theory to the animal phenomena.

11. The analysis of urine, and of urinary calculi, by the same gentleman, has been much admired on the Continent, and no doubt contains many important facts; but hardly any important addition is made in it to the dissertation of Dr Wollaston on the same subject, which had been already published in the Philosophical Transactions. To this very important paper no allusion whatever is made, yet they could hardly be ignorant of it, as they quote Dr Pearson's essay on the same subject, which had been published in the same work.

12. Their experiments on the combustion of bodies in oxymuriatic acid gas, and the detonations which take place when hyperoxymuriate of potash and a combustible substance are mixed together and struck upon an anvil, are curious, though they add but little to the improvement of the theory of chemistry.

13. Their method of obtaining barytes in a state of purity, by exposing the nitrate of barytes to a red heat in a porcelain crucible, is a good one; and is by far the easiest way to procure that earth in a state of tolerable purity.

14. Their theory of the formation of sulphuric ether, by the action of sulphuric acid, is plausible; and at least as likely to be true as any other explanation which has been

hitherto offered; but they were wrong in attempting to extend that theory to the formation of ether in general. We now know that the nitric, and muriatic, and acetic ether, are formed in quite a different manner.

15. They ascertained by experiment that the three liquids, known by the names of pyromucous, pyrolignous, and pyrotartarous acids, are nothing else than vinegar holding in solution a portion of empyreumatic oil.

16. They ascertained the presence of phosphate of magnesia in the bones of all animals.

17. Their experiments upon crude platina were not so successful. They detected in it the presence of a new metal. But as they in fact confounded the two metals of Tennant, the osmium and iridium, together, all their observations were either erroneous, or so confused that it was impossible to disentangle the truth from them.

18. Their experiments on the bitter principle extracted from indigo, and the detonating property which it possesses, are curious. The subject was carried further by Hatchett and Chevreul.

19. They were unsuccessful in their attempts to detect the presence of fluoric acid in bones; though this was afterwards successfully executed by Berzelius.

20. They discovered a quantity of uncombined phosphorus in the melts of fishes. They showed likewise an analogy between the pollen of the antheræ of some flowers, and the seminal fluid of animals.

21. They detected in the common onion the presence of a considerable quantity of saccharine matter, and showed by experiment, that this saccharine matter was converted into manna by a spontaneous change which it underwent. They found, at the same time, that manna is incapable of undergoing the vinous fermentation, and, of course, that it does not yield alcohol.

22. They ascertained the properties of animal mucus, and showed that it differed from all other animal substances.

23. These, though only a small number of the chemical papers published by Fourcroy, are by far the most important. We have no means of determining what portion of each belongs to Fourcroy, and what to Vauquelin; but there is one merit, at least, which cannot be refused Fourcroy, and it is no small one. He formed and brought forwards Vauquelin, and proved to him ever afterwards a most steady and indefatigable friend. This is bestowing no small panegyric on

his character; for it would have been impossible to have retained such a friend through all the horrors of the French revolution, if his own qualities had not been such as to merit so steady an attachment. I have taken no notice of the labours of M. de Fourcroy in the chemical part of the Encyclopedie Methodique, though they are rather voluminous, because I conceive them of inferior importance to those which I have noticed.

ART. II.—*On the Action of finely-divided Platinum on Gaseous Mixtures, and its Application to their Analysis.* By WILLIAM HENRY, M. D. F. R. S. [*Philos. Trans.*]

SEVERAL years have elapsed since the President of the Royal Society, in the further prosecution of those researches on flame which had already led him to the most important practical results, discovered some new and curious phænomena in the combustion of mixed gases, by means of fine wires of platinum introduced into them at a temperature below ignition. A wire of this sort being heated much below the point of visible redness, and immersed in a mixture of coal gas and oxygen gas in due proportions, immediately became white-hot, and continued to glow until all that was inflammable in the mixture was consumed. The wire, repeatedly taken out of the mixture and suffered to cool below the point of redness, instantly recovered its temperature on being again plunged into the mixed gases. The same phænomena were produced in mixtures of oxygen with olefiant gas, with carbonic oxide, with cyanogen, and with hydrogen; and in the last case there was an evident production of water. When the wire was very fine, and the gases had been mixed in explosive proportions, the heat of the wire became sufficiently intense to cause them to detonate. In mixtures which were non-explosive from the redundancy of one or other gas, the combination of their bases went on silently, and the same chemical compounds were formed as by their rapid combustion.

Facts analagous to these were announced in the autumn of last year by Professor Dœbereiner, of Jena, with this additional and striking circumstance,—that when platinum in a spongy form is introduced into an explosive mixture of oxygen and hydrogen, the metal, even though its temperature had not been previously raised, immediately glows, and causes the union of the two gases to take place sometimes silently, at others with detonation. It is remarkable, however,

that platinum in this form, though so active on mixtures of oxygen and hydrogen, produces no effect, at common temperatures, on mixtures of oxygen with those compound gases, which were found by Sir Humphry Davy to be so readily acted upon by the heated wire. Carbonic oxide appears indeed, from the statement of MM. Dulong and Thenard, to be capable of uniting with oxygen at the temperature of the atmosphere, by means of the sponge; but though this is in strictness true, yet the combination, in all the experiments I have made, has been extremely slow, and the due diminution of volume has not been completed till several days have elapsed. On mixtures of olefiant gas, of carburetted hydrogen, or of cyanogen, with oxygen, the sponge does not by any duration of contact exert the smallest action at common temperatures.

It was this inefficiency of the platinum sponge on the compounds of charcoal and hydrogen in mixture with oxygen, while it acts so remarkably on common hydrogen, and also, though slowly, on carbonic oxide, that suggested to me the possibility of solving by its means some interesting problems in gaseous analysis. I hoped more especially to be able to separate from each other the gases constituting certain mixtures, to the compositions of which approximations only had been hitherto made, by comparing the phænomena and results of their combustion with those which ought to ensue, supposing such mixtures to consist of certain hypothetical proportions of known gases. It might, for instance, be expected that from a mixture of hydrogen and carburetted hydrogen with oxygen, the platinum sponge would cause the removal of the hydrogen, leaving the carburetted hydrogen unaltered. To ascertain this and a variety of similar facts, I made artificial mixtures of the combustible gases in known volumes; and submitted them, mixed with oxygen, sometimes to contact with the sponge, and sometimes with the balls made of clay and platinum, described by Professor Dœbereiner.

SECTION I.

ON THE ACTION OF FINELY-DIVIDED PLATINUM ON GASEOUS MIXTURES AT COMMON TEMPERATURES.

1. *Mixtures of Hydrogen and Olefiant Gases with Oxygen.*

When to equal volumes of olefiant gas, and an explosive mixture (which is to be understood, whenever it is so named, as consisting of two volumes of hydrogen and one of oxygen

gases,) one of the platinum balls, recently heated by the blow-pipe, and allowed to cool during eight or ten seconds, is introduced through mercury, a rapid diminution of volume takes place; the whole of the hydrogen and oxygen gases is condensed; but the olefiant gas is either not at all or very little acted upon. In a few experiments when the tube was narrow, and the quantity of mixed gases small, the olefiant gas escaped combustion entirely; but, in general, an eighth or tenth of it was converted into water and carbonic acid. It is difficult, however, to state the precise proportion of any gas which, when added to an explosive mixture, renders the latter insensible to the action of the balls or sponge; for much depends on their temperature when introduced into the gaseous mixture, the diameter of the containing vessel, and other circumstances, which, in comparing different gases, should be so regulated as to be equal in every case.

When the proportions of the gases are changed, so that the explosive mixture exceeds in volume the olefiant gas, there is a more decided action upon the latter, manifested by an increased production of carbonic acid. Thus, for example, the explosive mixture being to the olefiant as $2\frac{1}{2}$ to 1, about one-fourth of the olefiant gas was consumed; and by increasing the proportion of the explosive mixture the olefiant gas was still more acted upon. On using oxygen sufficient to saturate both the hydrogen and the olefiant gases, the ball acted much more rapidly. In several instances it became red-hot; all the hydrogen was consumed; and the whole of the olefiant gas was changed into water and carbonic acid. In this case the use of the sponge is inadmissible, as it kindles the gases, and occasions their detonation.

2. *Mixtures of Hydrogen and Carburetted Hydrogen Gases with Oxygen.*

When carburetted hydrogen, procured from stagnant water, was added to an explosive mixture, in various proportions between equal volumes, and ten of the former to one of the latter, the action of the hydrogen and oxygen on each other took place as usual, on admitting one of the balls. When, reversing the proportion, the explosive mixture was made to exceed the carburetted hydrogen, but not more than four or five times, the latter gas was entirely unchanged. With a larger proportion of the explosive mixture, carbonic acid was always found to have been produced; but still the carbu-

retted hydrogen was very imperfectly consumed; and fully three-fourths of it were generally found to have escaped unburned.

When to a mixture of hydrogen and carburetted hydrogen, oxygen enough was added to saturate both gases, the effect of the sponge was found to vary with the proportion of the simple hydrogen. In several cases, where the hydrogen did not exceed the carburetted hydrogen more than four times, the latter gas remained unchanged; when in larger proportion, there was a decided action upon the carburetted hydrogen. But it was much more easy to regulate the action of the balls upon such a mixture so as to act upon the hydrogen and oxygen only, than in the case of olefiant gas, which, under similar circumstances, is always more largely converted into water and carbonic acid.

3. *Mixtures of Hydrogen and Carbonic Oxide with Oxygen.*

The addition of one volume of carbonic oxide to two volumes of an explosive mixture produces a distinct effect in suspending the action of the platinum balls, and even of the spongy metal itself. The action of the gases upon each other still, however, goes on slowly, even when the carbonic oxide exceeds the explosive mixture in volume; and after the lapse of a few days the oxygen is found to have disappeared, and to have partly formed water, and partly carbonic acid. I made numerous experiments to ascertain whether the oxygen under these circumstances of slow combustion, is divided between the carbonic oxide and the hydrogen in proportions corresponding to the volumes of those two gases. The combustible gases being in equal volumes, and the oxygen sufficient to saturate only one of them, it was found that the oxygen which had united with the carbonic oxide was to that which had combined with the hydrogen as about 5 to 1 in volume. Increasing the carbonic oxide, a still larger proportion of oxygen was expended in forming carbonic acid. On the contrary, when the hydrogen was increased, a greater proportional quantity of oxygen went to the formation of water. But it was remarkable, that when the hydrogen was made to exceed the carbonic oxide four or five times, less oxygen in the whole was consumed than before; the activity of the carbonic oxide appearing to have been diminished, without a corresponding increase in that of the hydrogen.

In cases where the proportion of the carbonic oxide to the explosive mixture was intentionally so limited that the plati-

num ball was capable of immediately acting upon the latter, the carbonic oxide was always in part changed into carbonic acid, the more abundantly as its volume was exceeded by that of the explosive mixture. Increasing the oxygen, so that it was adequate to saturate both gases, and causing the hydrogen to exceed the carbonic acid in volume, a speedy action was always exerted by the ball, and the whole of the combustible gases was silently converted into water and carbonic acid. The introduction of the platinum sponge into such a mixture was almost always found to produce detonation.

4. *Mixtures of Hydrogen and Cyanogen with Oxygen.*

When one of the platinum balls, after being recently heated, is introduced into cyanogen and explosive mixture in equal volumes, no apparent action takes place. With half a volume of cyanogen there is a slight diminution; and as we reduce the proportion of that gas, the action of the elements of the explosive mixture on each other becomes more and more distinct. There is not, however, as with carbonic oxide, any production of carbonic acid; but in the course of a few minutes, the inside of the tube becomes coated with a brownish substance, soluble in water, and communicating to it the same colour; having a smell resembling that of a burnt animal substance, and yielding ammonia on the addition of a drop or two of liquid potash. It was produced in too small a quantity to enable me to submit it to a more minute examination; but its characters appeared to resemble those of a product obtained by M. Gay-Lussac by mixing cyanogen with ammoniacal gas.

If oxygen be added to a mixture of hydrogen and cyanogen, in quantity sufficient to saturate both the gases, it is still necessary, in order that an immediate effect should be produced by the sponge, that the hydrogen should exceed the cyanogen in volume. A decided action then takes place; an immediate absorption ensues; fumes of nitrous acid vapour appear, which act on the surface of the mercury; and after removing the nitrous acid by a drop or two of water, and transferring the gas into a dry tube, carbonic acid is found to have been produced, equivalent in volume to double that of the cyanogen.

5. *Effect of adding various other Gases to an Explosive Mixture of Hydrogen and Oxygen.*

It had been already ascertained by Professor Dœbereiner, that one volume of oxygen diluted with 99 volumes of nitrogen is still sensible, when mixed with a due proportion of hydrogen, to the action of the sponge. Carbonic acid also, even (I find) when it exceeds the explosive mixture ten times, retards only in a slight degree the energy of the sponge. Oxygen, hydrogen, and nitrous oxide gases, when employed to dilute an explosive mixture, are equally inefficient in preventing the mutual action of its ingredients. Ammonia may be added in ten times the volume of the explosive mixture, and muriatic acid gas in six times its volume, with no other effect than that of rendering the action of the sponge less speedy.

6. *Mixtures of Carbonic Oxide and Carburetted Hydrogen with Oxygen.*

When mixtures of these gases are exposed to the sponge, the carburetted hydrogen seems to stand entirely neutral. The carbonic oxide is converted into carbonic acid, in the same gradual manner as if it had been mixed with oxygen only; and the carburetted hydrogen remains unaltered.

7. *Mixtures of Hydrogen, Carburetted Hydrogen, and Carbonic Oxide with Oxygen.*

In mixtures of these gases, it is of little consequence whether the oxygen be sufficient for the hydrogen and carbonic oxide only, or be adequate to the saturation of all three. The circumstance which has the greatest influence on the results of exposing such mixtures to the sponge, is the proportion which the simple hydrogen bears to the other gases, and especially to the carbonic oxide; for in order that there may be any immediate action, the former should exceed the latter in volume. In that case the hydrogen is converted into water, and the carbonic oxide into carbonic acid; but the carburetted hydrogen, unless the excess of hydrogen be very considerable, remains unaltered. If the proportion of hydrogen be so small that no immediate action is excited by the sponge, the ingredients of the mixture nevertheless act slowly upon each other; and after a few days the whole of the hydrogen and carbonic oxide are found to have united with oxygen, and the carburetted hydrogen to remain of its original volume.

8. *Mixtures of Hydrogen, Carbonic Oxide, and Olefiant Gases with Oxygen.*

When the oxygen, in a mixture of these gases, is sufficient to saturate the two first only, and the proportion of hydrogen is so adjusted that the action of the sponge is not very energetic, the hydrogen and carbonic oxide only are acted upon; but if the diminution of volume, which the sponge produces, be rapid and considerable, part of the olefiant gas is converted into water and carbonic acid. This effect on olefiant gas takes place still more readily if the oxygen present be adequate to the saturation of all three combustible gases.

It is remarkable, that if to a mixture of hydrogen, carbonic oxide, and oxygen, in such proportions that the sponge would act rapidly in producing combination, olefiant gas be added, the action of the gases on each other is suspended. Thus 20 measures of carbonic oxide, 31 of hydrogen, and 20 of oxygen, were instantly acted upon by the sponge; but the addition of 20 measures of olefiant gas to a similar mixture entirely suspended its efficiency. By standing fourteen days, rather more than half the carbonic oxide was acidified, and about one-twelfth of the hydrogen was changed into water, but the olefiant gas remained unaltered.

9. *Mixtures of Hydrogen, Carbonic Oxide, Carburetted Hydrogen, and Olefiant Gases with Oxygen.*

In mixtures of these four gases with oxygen, it was found by varying the proportion of hydrogen, that hydrogen and carbonic oxide are most easily acted upon; then olefiant gas; and carburetted hydrogen with the greatest difficulty. When the action of the sponge was moderate, only the hydrogen and carbonic oxide were consumed, or at most the olefiant gas was but partially acted upon. Adding more hydrogen, so as to occasion a more rapid diminution, the olefiant gas also was burned; but the carburetted hydrogen always escaped combustion, unless the hydrogen were in such proportion that the ball or sponge became red-hot.

From the facts which have been stated, it appears that when the compound combustible gases, mixed with each other, with hydrogen, and with oxygen, are exposed to the platinum balls or sponge, the several gases are not acted upon with equal facility; but that carbonic oxide is most disposed to unite with oxygen; then olefiant gas; and lastly, carburetted hydrogen. By due regulation of the proportion of hydrogen, it is possible to change the whole of the carbonic oxide into

carbonic acid without acting on the olefiant gas or carburetted hydrogen. With respect indeed to olefiant gas, this exclusion is attended with some difficulty, and it is generally more or less converted into carbonic acid and water. But it is easy, when olefiant gas is absent, so to regulate the proportion of hydrogen, that the carbonic oxide may be entirely acidified, and the whole of the carburetted hydrogen be left unaltered. This will generally be found to have been accomplished, when the platinum ball has occasioned a diminution of the mixture, at about the same rate as atmospheric air is diminished by nitrous gas when the former is admitted to the latter in a narrow tube.

SECTION II.

On the Effect of finely-divided Platinum on Gaseous Mixtures at increased Temperatures.

The effect of varying the proportion of free hydrogen to the compound combustible gases, on the degree of action which is excited by the platinum sponge, will perhaps admit of being explained, by examining the facts that have been stated, in connexion with the degrees of combustibility of the compound gases under ordinary circumstances. The precise degree of temperature at which any one of them burns is not known, on account of the imperfection of our present methods of measuring high degrees of heat. It has been ascertained, however, by Sir Humphry Davy, that at a heat between that of boiling mercury and that which renders glass luminous in the dark, hydrogen and oxygen gases unite silently, and without any light being evolved; that carbonic oxide is as inflammable as hydrogen; that olefiant gas is fired by iron and charcoal heated to redness; but that carburetted hydrogen, to be inflamed, requires that the wire should be white-hot. Now this is precisely the order in which the three compound gases require hydrogen to be added to them, in order to be rendered susceptible of being acted upon by the platinum sponge; carbonic oxide being acted upon with the smallest proportion of hydrogen, olefiant gas requiring more hydrogen, and carburetted hydrogen a still larger proportion. It is extremely probable then, that the temperature, produced by the union of the hydrogen and oxygen forming part of any mixture, is the circumstance which determines the combustible gases to unite, or not, with oxygen, by means of the sponge. It was desirable, however, to ascertain the exact temperature

at which each of those three gasses unites with oxygen with the intervention of the spongy platinum. For this purpose the gases, mixed with oxygen enough to saturate them, were severally exposed in small retorts containing a platinum sponge, and immersed in a mercurial bath, to a temperature which was gradually raised till the gases began to act on each other. In this way the following facts were determined.

1st. Carbonic oxide began to be converted into carbonic acid at a temperature between 300° and 310° Fahrenheit. By raising the temperature to 340°, and keeping it at that point for 10 or 15 minutes, the whole of the gas was acidified, the condensation of volume in the mixture being equivalent to the oxygen which had disappeared.

2d. Olefiant gas, mixed with sufficient oxygen, and in contact with the sponge, showed a commencement of decomposition at 480° Fahrenheit, and was slowly but entirely changed into carbonic acid by a temperature not exceeding 520° Fahrenheit. MM. Dulong and Thenard state the same change to take place at 300° Cent. = 572° Fahrenheit; but having repeated the experiment several times, I find no reason to deviate from the temperature which I have assigned.

3d. Carburetted hydrogen, exposed under the same circumstances, was not in the least acted upon by a temperature of 555° Fahrenheit, the highest to which, by an Argand's lamp, I was able to raise the mercurial bath. This, however, must have been near the temperature required for combination; for on removing the retort from the mercurial bath, and applying a spirit lamp, at such a distance as not to make the retort red-hot, a diminution of volume commenced, and continued till all the carburetted hydrogen was silently converted into water and carbonic acid.

4th. Cyanogen, similarly treated, was not changed at a temperature of 555° Fahrenheit; and on applying the flame of a spirit lamp to the tube, it produced no action till the tube began to soften.

5th. Muriatic acid gas, mixed with half its volume of oxygen, began to be acted upon at 250° Fahrenheit. Water was evidently formed; and the disengaged chlorine, acting upon the mercurial vapour in the tube, formed calomel, which was condensed, and coated its inner surface.

6th. Ammoniacal gas, mixed with an equal volume of oxygen, showed a commencement of decomposition at 380° Fahrenheit. Water was also in this case distinctly generated;

and at the close of the experiment nothing remained in the tube but nitrogen and the redundant oxygen.

I proceeded, in the next place, to examine the agency of finely-divided platinum at high temperatures, on those mixtures of gases, which are either not decomposed, or are slowly decomposed, at the temperature of the atmosphere.

When carbonic oxide and hydrogen gases, in equal volumes, mixed with oxygen sufficient to saturate only one of them, were placed in contact with the sponge, and gradually heated in a mercurial bath, the mixture ceased to expand between 300° and 310° Fahrenheit, and soon began to diminish in volume. On raising the temperature to 340°, and keeping it some time at that point, no further diminution was at length perceptible. From the quantity of carbonic acid remaining at the close of the experiment, it appeared that four-fifths of the oxygen had united with the carbonic oxide, and only one-fifth with the hydrogen. When four volumes of hydrogen, two of carbonic oxide, and one of oxygen, were similarly treated, the hydrogen, notwithstanding its greater proportional volume, was still found to have taken only one-fifth of the oxygen, while four-fifths had combined with the carbonic oxide. These facts show that at temperatures between 300° and 340° Fahrenheit, the affinity of carbonic oxide for oxygen is decidedly superior to that of hydrogen; as, from the experiments before described, appears to be the case, also at common temperatures.

But a similar distribution of oxygen, between carbonic oxide and hydrogen, does not take place, when those three gases are fired together by the electric spark. This will appear from the following table, in which the three first columns show the quantities of gases that were fired, and the two last, the quantities of oxygen that were found to have united with the carbonic oxide and with the hydrogen.

	Before Firing.			*After Firing.*	
	Measure of Carb. Oxide.	Measure of Hydrogen.	Measure of Oxygen.	Oxygen to Carb. Oxide.	Oxygen to Hydrogen.
Exp. 1.	40	40	20	6	14
2.	40	20	20	12	8
3.	20	40	20	5	15

When equal volumes of carbonic oxide and hydrogen gases, mixed with oxygen sufficient to saturate only one of them, were exposed in a glass tube to the flame of a spirit lamp, without the presence of the sponge, till the tube began to soft-

en, the combination of the gases was effected without explosion, and was merely indicated by a diminution of volume and an oscillatory motion of the mercury in the tube. At the close of the experiment, out of twenty volumes of oxygen, eight were found to have united with the carbonic oxide, and twelve with the hydrogen--proportions which do not materially differ from the results of the first experiment in the foregoing table. At high temperatures, then, the attraction of hydrogen for oxygen appears to exceed that of carbonic oxide for oxygen; at lower temperatures, especially when the gases are in contact with the platinum sponge, the reverse takes place, and the affinity of carbonic oxide for oxygen prevails.

Extending the comparison to the attraction of olefiant and hydrogen gases for oxygen at a red heat, I found that when six volumes of olefiant, six of hydrogen, and three of oxygen were heated by a spirit lamp till the tube softened, a silent combination took place as before; all the oxygen was consumed, but only half a volume had been expended in forming carbonic acid, which indicates the decomposition of only one quarter of a volume of olefiant gas. On attempting a similar comparison between carbonic oxide and olefiant gas, by heating them with oxygen in the same proportions, the mixture exploded as soon as the glass became red-hot, and burst the tube.

The property inherent in certain gases, of retarding the action of the platinum sponge, when they are added to an explosive mixture of oxygen and hydrogen, is most remarkable in those which possess the strongest attraction for oxygen; and it is probably to the degree of this attraction rather than to any agency arising out of their relations to caloric, that we are to ascribe the various powers which the gases manifest in this respect. This will appear from the following table the first column of which shows the number of volumes of each gas required to render one volume of an explosive mixture of hydrogen and oxygen uninflammable by the discharge of a Leyden jar; while the second column shows the number of volumes of each gas necessary in some cases to render one volume of an explosive mixture insensible to the action of the sponge, and in other cases indicates the number which may be added without preventing immediate combination. In the first column, the numbers marked with an asterisk were determined by Sir Humphry Davy; the remaining numbers in that column, and the whole of the second, are derived from my own experiments.

1 vol. of Explosive Mixture was rendered incapable of being inflamed by electricity when mixed with			Effect of adding the same gases to 1 vol. of Explosive Mixture on the action of the sponge.
*About	8 vol.	of Hydrogen	not prevented by many vol.
	6	Nitrogen	ditto.
*	9	Oxygen	not prevented by 10 vol.
*	11	Nitrous Oxide	ditto.
	1.5	Cyanogen	prevented by 1 vol.
*	1	Carb^{zd}. Hyd.	not prevented by 10 vol.
	4	Carb^{e}. Oxide	prevented by $\frac{1}{2}$ vol.
*	0.5	Olefiant Gas	prevented by 1·5 vol.
*	2	Muriatic Acid	not prevented by 6 vol.
	2	Ammonia	not prevented by 10 vol.
	3	Carbonic Acid	ditto.

From the foregoing table it appears that carbonic oxide produces the greatest effect, in the smallest proportion, to an explosive mixture of oxygen and hydrogen, in preventing the action of those gases on each other, when exposed to the sponge at temperatures below the boiling point of mercury. In general those gases which either do not unite with oxygen, or unite with it only at high temperatures, have little effect in restraining the efficiency of the sponge. There is an apparent exception, however, in cyanogen, which it would require more research than I have yet had time to devote to an object merely collateral, to reconcile it, if it be capable of being reconciled with the general principle.

From the fact that carbonic oxide, olefiant gas, and carburetted hydrogen, when brought to unite with oxygen by means of the platinum sponge, assisted by heat, undergo this change at different temperatures, it seemed an obvious conclusion, that by exposing a mixture of those gases with each other and with oxygen to a regulated temperature, the correct analysis of such mixtures might probably be accomplished. Mixtures of two or more of the combustible gases were therefore exposed, in contact with oxygen gas and the platinum sponge, in tubes bent into the shape of retorts, which were immersed in a mercurial bath. This bath was gradually heated to the required temperatures, and by proper management of the source of heat was prevented from rising above that degree.

1st. By subjecting 25 measures of carbonic oxide, 15 of olefiant gas, and 57 of oxygen, in contact with the sponge, to a heat which was not allowed to exceed 350° Fahrenheit till the diminution of volume ceased all the carbonic oxide was

converted into carbonic acid, and the olefiant gas remained in its original volume.

2d. By exposing in a similar manner 20 measures of carbonic oxide, 21 of carburetted hydrogen, and 36 of oxygen, to a temperature below 400° Fahrenheit, the carbonic oxide was entirely acidified; and on washing out the carbonic acid by liquid potash, the carburetted hydrogen was found unaltered, mixed with the redundant oxygen.

3d. A mixture of 10 measures of olefiant gas, 10 of carburetted hydrogen, and 58 of oxygen, being heated in contact with the sponge to 510° Fahrenheit, the olefiant gas was silently but entirely changed into carbonic acid, while the carburetted hydrogen was not at all acted upon.

4th. By acting with the sponge upon 42 measures of carburetted hydrogen, 22 of carbonic oxide, 22 of hydrogen, and 28 of oxygen, first at a temperature of 340° Fahrenheit, which was raised gradually to 480°, all the carbonic oxide was changed into carbonic acid, and all the hydrogen into water; but the carburetted hydrogen remained undiminished in quantity, and was found, after removing the carbonic acid, mixed only with the redundant oxygen. In this experiment the diminution of volume had continued some time before there was any perceptible formation of water, the attraction of carbonic oxide for oxygen appearing to prevail over that of hydrogen. The same precedency in the formation of carbonic acid is always apparent when carbonic oxide and hydrogen, mixed even with oxygen enough to saturate both gases, are raised to 350° Fahrenheit.

By thus carefully regulating the temperature of the mercurial bath, the action of oxygen upon several gases (carbonic oxide, olefiant, and carburretted hydrogen, for example,) may be made to take place in succession; and by removing the carbonic acid formed at each operation, it may be ascertained how much of each of the two first gases has been decomposed. The carburetted hydrogen indeed always remains unchanged, and its quantity must be determined by firing it with oxygen by the electric spark. If hydrogen also be present, it is difficult to prevent the olefiant gas from being partially acted upon; but this is of little consequence, as I had shown that it is easy to remove that gas in the first instance by chlorine. It may be remarked, that this method of operating on the aeriform compounds of charcoal gives more accurate results than rapid combustion by the electric spark, being never attended with that precipitation of charcoal which is often observed

when the gases are exploded with oxygen. A regulated temperature also effects the analysis of such mixtures much more correctly than the action of the sponge or balls, because in the latter case the heat produced is uncertain; and though sometimes adequate to the effect, yet there is always a risk that it may exceed or fall short of that degree which is required for the successful result of the analytic process.

From the facts which have been stated, I derive a method of obtaining carburetted hydrogen gas perfectly free from olefiant gas, hydrogen, and carbonic oxide, and mixed only with a little oxygen, which, had it been necessary to my purpose, might also have been separated. The early product of the distillation of pit-coal was washed with a watery solution of chlorine, and afterwards with liquid potash to remove a little chlorine that arose into the gas from the solution. The residuary gas was next heated with one fourth of its volume of oxygen at the temperature of 350° Fahrenheit, in contact with the sponge, which converted the carbonic oxide into carbonic acid, and the hydrogen into water. The carbonic acid being removed by liquid potash, there remained only the carburetted hydrogen, the redundant oxygen, and a very minute quantity of nitrogen introduced by the latter gas. Hitherto I have prepared this gas only in a small quantity; but it would be easy to extend the scale of the operation, and to remove the excess of oxygen by obvious methods.

SECTION III.

Application of the Facts to the Analysis of Mixtures of the combustible Gases in unknown Proportions.

At an early period of the investigation described in the first section, I proceeded to apply the facts of which I was then possessed, to the analysis of a mixture of gases in unknown proportions. For this purpose, I caused a quantity of gas to be collected from coal, by continuing the application of heat to the retorts two hours beyond the usual period and receiving the gas into a separate vessel. Gas of this quality was purposely chosen, because from former experience I expected it to contain free hydrogen, carbonic oxide, and carburetted hydrogen, but no olefiant gas; the production of which is confined to the early stages of the process. After washing it, therefore, with liquid potash, to remove a little

carbonic acid, and ascertaining its specific gravity, when thus washed, to be 308, I proceeded at once to subject it to the new method of analysis.

Having ascertained by a previous experiment with Volta's eudiometer that 10 volumes of the gas required for saturation 9 volumes of oxygen, I mixed 43 measures with 43 of oxygen (= 41 pure), and passed a platinum ball, which had been recently heated, into the mixture. An immediate diminution of volume took place, attended with a production of heat and formation of moisture. The residuary gas, cooled to the temperature of the atmosphere, measured 43·5 volumes. Of these, 4·5 were absorbed by liquid potash, indicating 4·5 carbonic acid, equivalent to 4·5 carbonic oxide; the rest, being fired in a Volta's eudiometer, with an additional quantity of oxygen, gave 11 volumes of carbonic acid; the diminution being 22 volumes, and the oxygen consumed 22 also—circumstances which prove that 11 volumes of carburetted hydrogen were consumed by this rapid combustion. But of the loss of volume first observed (viz. 86 — 43·5 = 42·5), 2·25 are due to the carbonic acid formed; and deducting this from 42·5 we have 40·25, which are due to the oxygen and hydrogen, converted into water; and 40·25 × $\frac{2}{3}$ = 26·8 shows the hydrogen in the original gas. But the sum of these numbers (26·8 + 4·5 + 11) being less by 0·7 than the volume of gas submitted to analysis, we may safely consider that fraction of a measure to have been nitrogen. The composition then of the mixture will stand in volumes as follows:

Hydrogen	26·8	62·32
Carbonic oxide	4·5	10·50
Carburetted hydrogen . .	11·0	25.56
Nitrogen	0·7	1·62
	43.0	100.0

On calculating what should be the specific gravity of a mixture of gases in the above proportions, it was found to be 303; which coincides as nearly as can be expected with the actual specific gravity of the gas submitted to analysis, viz. 308. To place the correctness of the results beyond question, I mingled the gases in the above proportions, and acted on the artificial mixture in the same manner as on the original gas; when I had the satisfaction to find that the analytical process again gave the true volumes with the most perfect correctness for the hydrogen and carbonic oxide, and within the fraction of a measure for the carburetted hydrogen. Not-

withstanding this successful result, which was twice obtained, I should still prefer, for the reason which has been stated, having recourse to a temperature carefully regulated, for the analysis of similar mixtures, in all cases where the hydrogen is in moderate proportion, and where great accuracy is desirable. Whenever (it may again be remarked) olefiant gas is present in a mixture, it should always be removed by chlorine before proceeding to expose the mixture to the agency of the spongy metal.

It can scarcely be necessary to enter into further details respecting methods of analysis, the application of which to particular cases must be sufficiently obvious from the experiments which have been described on artificial mixtures. The apparatus required is extremely simple, consisting, when the balls are employed, of graduated tubes of a diameter between 0·3 and 0·6 of an inch, or, when an increased temperature is used, of tubes bent into the shape of retorts, of a diameter varying with the quantity of gas to be submitted to experiment, which may be from half a cubic inch to a cubic inch or more. These when in use may be immersed in a small iron cistern containing mercury, and provided with a cover in which are two holes, one for the tube and the other for the stem of a thermometer, the degrees of which are best engraved on the glass.

By means of these improved modes of analysis, I have already obtained some interesting illustrations of the nature of the gases from coal and from oil. I reserve, however, the communication of them till I have had an opportunity of pursuing the inquiry to a greater extent, and especially of satisfying myself respecting the exact nature of the compound of charcoal and hydrogen, discovered some years ago by Mr Dalton, in oil gas and coal gas, which agrees with olefiant gas in being condensible by chlorine, but differs from it in affording more carbonic acid and consuming more oxygen.

ART. III.—*Facts proving the Efficacy of Sir H. Davy's Method of protecting the Copper of Ships by Electrochemical Action.* Extracted from the Letters of a Correspondent, and Dr Stewart Traill. [*An. Philos.*]

1. THE *Carnebrea Castle*, an Indiaman, belonging to Messrs Wigram, of 650 tons burden, was protected last spring by a

quantity of iron in four portions, two on the bow and two on the stern, equal to form $\frac{1}{100}$ to $\frac{1}{110}$ part. She has since made the voyage to India, and was for some time in the Ganges.

She appeared bright and clean during the whole of the voyage out and home; some mud collected on her bottom in the Ganges; but immediately disappeared when she began to sail. She was put into dry dock about a fortnight ago, and her bottom examined by Sir H. Davy, the proprietors, and various other persons. *Every part* of her bottom was bright and clean, without a single adhesion of any kind, and as far as could be judged from the smoothness and appearance of the copper, it had not been at all worn by any chemical corrosion. The iron, which was about an inch and a half in thickness, is considered a sufficient protector for two voyages more.

2. The *Elizabeth* yacht, belonging to the Earl of Darnley, was protected by two pieces of malleable iron in the stern, in May last, equal to about $\frac{1}{125}$ of the surface of the copper. After being employed in sailing during the summer, she was examined in November, when her bottom was found free from adhesions of any kind, and apparently untouched. The copper was bright, and even the nails not tarnished. In the course of the summer a few small barnacles had adhered to the rust of the iron, which were easily and immediately washed off; but no weed or shell-fish had ever fixed on the copper, which appeared in the same state as when she left the dock.

The following examples we owe to the kindness of Dr Traill:

The ship *Huskisson*, belonging to Mr Horsfall, was lately in dock after a voyage to and from Demerara, where she lay some weeks, in a river remarkably favourable to the adhesion of parasitical animals and weeds; yet, when I examined this vessel, her copper appeared perfectly clean, as far as it could be seen, when she was purposely *set by the stern*, in unloading, in order to show her copper at the *bows* as low as possible. The captain stated that, before coming into port, while yet in clear water, he had seen her bottom even to the keel; and it seemed to him quite clean. This ship was *defended* by two bars of malleable iron, bolted along the sides of her keel by copper fastenings, which covered about $\frac{1}{90}$ of the surface of her copper.

The *Elizabeth*, a vessel defended exactly in the same manner, with metals in the same proportions, had made the same voyage. Both had been newly coppered when they last left

Liverpool; and the Elizabeth's copper appeared equally clean as that of the *Huskisson* when unloaded; but as she did enter a *graving* dock, we cannot absolutely say whether she was quite clean, especially as the copper of the Dorothy (about to be mentioned) appeared equally so, until she was seen in the graving dock, when the flat part of her bottom was found to be quite covered with barnacles. The copper of the *Huskisson*, there is reason to believe, was *perfectly clean*, as was proved in the next case.

The ship *Dee*, a very large vessel belonging to my relative, Mr Sandbach. This ship was newly coppered about twelve months ago, and a bar of malleable iron, about $\frac{7}{8}$ of an inch thick, and three inches broad, was fastened on each side of the keel by *iron* spikes. It covered about $\frac{1}{90}$ of the surface of her copper. Since that period she has made two voyages to Demerara, and was, at the conclusion of the last, put into a graving dock, when her copper was found perfectly free from corrosion, and there were scarcely any substances adhering to it, except a very few minute barnacles, near the keel *fore* and *aft*. This case shows that *over defence* was not the cause of the foulness of the bottom of the *Tickler;* for both in this vessel and in the *Huskisson*, the proportion of iron to the copper was greater than in that ship. The iron spikes employed to fasten the iron on the keel of the Dee, were so much corroded, as to endanger the falling off of the bars; copper nails are, therefore, to be preferred.

The Dorothy.—Dr Traill states, that the following particulars of the Dorothy's outfit and return, were communicated to him by his intelligent friend, Mr Horsfall, one of the owners of the ship in the beginning of May:

"The *Dorothy* had been coppered about a year, and had made one voyage to Bombay and back to this port, when, in May, 1824, it was determined to place bars of iron four inches broad, and one inch thick, along her keel, covering about $\frac{1}{70}$ part of the copper, in the expectation that the iron would at least so far preserve the copper from corrosion that it might be permitted to run a second voyage to India without being renewed, which can seldom be done with perfect safety. The iron extended from one end of the keel to the other, and was fastened on with copper nails with large heads. The Dorothy, thus defended, sailed again for Bombay in June, and returned to Liverpool about a month since. She was put into the graving dock yesterday, (May 3,) and an examination of her bottom took place as soon as the water had left her.

The copper appeared no more reduced than at the termination of the first voyage. The iron was diminished generally about $\frac{3}{4}$ of an inch in breadth, and from $\frac{1}{4}$ to $\frac{1}{2}$ an inch in thickness. At the ends of the vessel, for about two or three feet, the iron was much more reduced than at any other part. It was covered with the usual rust, not at all resembling cast iron, under similar circumstances. The flat of the ship's bottom from end to end, and from six to eight feet in breadth, was full of *fleshy barnacles*, (lepas anatiferà) of uncommon length, and a few of the large hard shell species (balanus tintinnabulum)."*

What remains of the iron is still considered as sufficient protection for a third voyage to India, and "it appears only to be necessary to drive the large copper nails up a little to secure the iron bars for the next voyage."

Note by Dr Traill.—We remarked that the specimens of the *lepas anatifera* were considerably larger on the *starboard* than on the larboard side of the ship. On noticing this to the captain, he informed us that the *larboard* had been the *lee* side of the vessel, almost constantly during the passage to Europe, and consequently most deeply immersed in the water—a circumstance in the economy of these animals not unworthy of notice.

It is evident that in all these last cases, particularly in the ship Dorothy, the proportion of iron has been too large, and the quantity of calcareous earth on the bottom of this ship proves that the electro-negative action has been in excess.

C.

ART. IV.—*On the Transportation of Fish from salt to fresh Water.* By J. MACCULLOCH, M. D. F. R. S., &c. [*Journ. Roy. Inst.*]

You expressed a desire to know the progress which has been made in the transplantation of fish from salt to fresh water, since the period at which I communicated the paper on that subject to your Journal. Mr Arnold, who has carried on these experiments, at my wish, with great zeal, has succeeded in adding many more to the list, and, both in respect to the physical fact, and to the question of economy, the success has been far greater than any one was willing to believe.

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* Sulphuric acid was used to loosen and detach the shells.

The list of the additional fish will be seen by comparing that which is appended to this letter, with the former one; and as the subject has excited considerable attention, you will perhaps not object to a statement which may attract even more, by presenting, in the form of a prospectus, the essential facts and arguments. It is only by placing them in this form that they are very likely to produce the effect which appears desirable. I may now, however, subjoin some remarks which could not well find their place in such a statement, and which have been the result of more experience and attention. It is certain that the flavour of every fish which has yet been tried has been improved, and I can vouch for the superiority of the basse, the mullet, the loach, the atherine, and the sole, from the pond, to those from the sea. This might be expected, for it is what happens notedly with respect to oysters.

The sole becomes twice as thick as a fish of the same size from the sea, and its skin also becomes extremely dark, or nearly black.

The plaise also increases materially in thickness, and loses its spots. In some cases, it appeared three times as thick as in the sea. The basse also turns much thicker, and improves in delicacy.

The mullet almost ceases to grow in length, but enlarges in breadth, and presents a much deeper layer of fat.

Crabs and prawns have found their own way into the pond, as have loaches, and some other small fish; and while, formerly, there were none of the former two, the water is now absolutely swarming with them. Thus also, apparently, the eels have multiplied; as it is now easy to take a cartload at once, where formerly a dozen or two was a large capture. I have thus, also, more distinctly ascertained, and to the satisfaction of Cuvier, who had been unwilling to admit it, that there are two species of fresh water eel, distinguished by the comparative acuteness and breadth of the nose.

I have lastly to add an observation inadvertently omitted in the former communication, which may be used as an *à priori* argument for the possibility of this transplantation. It is, that oxygen is much more easily disengaged from fresh than from salt water. Consequently, the act of respiration ought to be easier in the former than in the latter; and, therefore, it is not to be presumed, as it has been, that sea-fish cannot respire fresh water.

As I have given the shad without its Linnæan name, I think it right to add, that our shad is yet unnamed; because the

Clupea Alosa is the Alose of the French, common in the Seine, and on the other coast of Normandy; a fish as good as our own shad is detestable, and a decidedly different species of this troublesome and ill understood genus. If I have given the vulgar term rock-fish, it is because I wish to reject the term urasse, for the present, as it stands a species; whereas the whole of this genus (labras) is still in extreme confusion, and in one, which I hope to aid in rectifying, with the assistance of Cuvier's materials and our own species.

Prospectus of a Plan for Preserving and Rearing Fish for the London Market.

From various observations and experiments, of which evidence is subjoined, it has been found that sea-fish will live and thrive, and also breed, in ponds and enclosures; and, with regard to many, it also appears that it is indifferent whether the water is salt, or fresh, or brackish, or alternately fresh and salt.

It is also found that they may be fed in such enclosures, if necessary, as our domestic animals are; but that if sufficient numbers and kinds are placed together, they feed each other without requiring further care.

It is further observed, that every, or almost every species, improves in flavour and quality, as oysters are known to do, under transplantation.

It is well known that, of all the fish brought to market, a very small proportion is in good condition, the rest being apparently ill fed; and hence the number of bad fish so well known to fishmongers.

It is much better known that, from bad weather, or other causes, the supply of the market is very irregular. Thus the public suffers when the supply is short, and the merchant when there is a glut. It is not uncommon for a glut to come in London when the town is empty; and, on the contrary, for it to want fish when full.

The proposed plan, if executed, would bring the fish within our own power, to be taken alive when wanted, and, from being better fed, in greater perfection and more uniformly good. It would be like taking stalled oxen, instead of wild Scotch cattle. It would also enable the merchants to regulate the supply by the demand, and thus to satisfy better the public and themselves. The trade would be steady instead of precarious; as the prices to the public would also be. It would be conducted, directly between the public and the grower, or

with the intervention of one retailer only, and thus a heavy cause of complaint be removed. Lastly, the public would be always sure of fish, and it would be sure of them at moderate prices.

Such are the proposed advantages. As far as the species that breed in such confinement, the fish would reproduce themselves, or a pond of this nature would resemble a common fish-pond. For those that will not, if there are any such, the ponds would be mere repositories for keeping alive, till there was a demand, the fish brought into them from the sea. They would also be feeding-places, allowing bad fish to improve. Thus far, the fisheries would go on as before, and the fishing-trade would consequently not be injured. It would be increased, on the contrary; because, with a better-regulated market and more moderate prices, the consumption would be augmented.

There can be no objection, therefore, on the score of injury to the fisheries. The plan is, virtually, one to preserve fish alive after being taken, instead of suffering them to waste, to render them better in quality, and to supply them more regularly.

The plan, therefore, is, to enclose, in any convenient part of the Thames, (since the quality of the water is proved to be indifferent) a space sufficient for the purpose. A dock, or an excavation in the nature of one, would be unnecessary, as the water itself, in many places not navigable, might be enclosed by a palisade. In this, the fish would be received from the fishermen, by means of well-boats, alive. Those which chanced to die would become the food of others. Many would breed, as they have been found to do, and thus also produce food. But they might also be fed, by means of butcher's offal, or other matters easily procured in a great city, as was the practice of the ancient Romans.

From the enclosure, the fish would be taken by nets, the kinds in demand, and the quantity, selected, and the bad fish also returned for improvement. A steam-boat would supply them to London daily, and to any market which might be established; and they might even be brought up alive, so that the unsaleable ones would not be lost.

The only capital required to be sunk, or advanced, would be in purchasing and enclosing a tract of water, and in the general establishment; possibly in stocking the pond. It could not be very large; but no estimate is now pretended to be given, nor any place pointed out; that needs not, however,

be very near to London, as a steam-boat would approximate any distances. After this, the fish would be purchased from the fishermen by contract; and the establishment beginning to sell, would then pay its way.

The details of evidence in support of the practicability of this scheme, are the following:

There are three or four sea-ponds in Scotland where fish are thus kept; one in Orkney, belonging to Mrs Stewart, one on the Firth of Forth, belonging to Sir Robert Preston; and one in Galloway, belonging to Mr Macdowall.

On the Greek coast of the Adriatic, at Missolonghi and elsewhere, the same has been practised from immemorial time. It is the current practice also of Bermuda, where the inhabitants subsist chiefly on fish.

These are sea-ponds, as the water is salt. But in Sicily, from the most ancient times also, the natives transport lobsters and crabs to a fresh-water and muddy lake, for the purpose of improving them, as they also do mullet.

With respect to fresh waters, we have evidence of the power of keeping and improving sea-fish in them, from the practice of the ancient Romans. From the testimony of Columella, and the other writers, "de Re Rustica," it was the practice of the Roman farmers, in the earliest days of the Republic, to go down to the sea and bring up the spawn of sea-fish to the fresh water lakes of Rome, where they multiplied and improved. It was a branch of farming. It became the amusement and luxury of the rich and great in the times of imperial Rome; enormous establishments of this nature were formed, and the fish were often fed at an expense which, as well as the value of the ponds, proves the great extent of these repositories.

Lastly, this plan has been recently put to the test under the direction of the writer of this note, in Guernsey, by Mr Arnold. In a pond of about four acres only, many sea-fish, which will be found in the following list, are now thriving, and all those which have had sufficient time have propagated; all have improved in quality, and many very remarkably. This pond was at first worthless, containing only a few eels; at present it produces a large rent, and can supply the market when the weather prevents the boats from going out. It is remarkable also, that, since the introduction of the sea-fish, the eels have multiplied a thousand fold, so as themselves to produce a considerable revenue. This proves that fish may be fed, merely by bringing different kinds together, as is the

case in nature. It may be added, that the evidence from this pond is peculiarly satisfactory, as far as relates to the indifference which sea-fish possess as to the quality of the water. Being embanked from the sea, and receiving an insufficient supply of fresh water in summer, it varies, so that while it is perfectly fresh in winter, it is nearly salt in very dry weather, and brackish in various degrees at intermediate periods. Here also, it is remarkable, that while the larger fishes have been placed there, many of the smaller ones, which formerly showed no such desire, have introduced themselves through crevices in the sea wall, and that it is, in particular, crowded with crabs and prawns.

It is now necessary to subjoin a list of the fishes which belonging naturally to the sea, have been found to live in fresh waters. Some of these have been forcibly introduced, others seek it for themselves. If the list is still limited, it is because the rest have not been tried; for no fish on which the experiment has been properly tried, has failed. When they have failed, it is because they were previously injured, or nearly killed, in the taking or the transportation. The star indicates those which have been forcibly naturalized in Mr Arnold's or some other pond.

Conger
Torsk
Sprat
Shad
Alose (of the French) clupea alosa
Greater Lamprey
Lesser Lamprey
Stickleback
Cottus quadricornis
Mullet
* Plaice
Flounder
Red Flounder, pleuronectes roseus.
White Whale
* Horse Mackerel
Pollack
Prawns
Cod
* Basse
Loach
Red Loach
* Smelt
* Atherine
* Rock-fish
* Cuckoo-fish
* Old Wife
* Sole
* Turbot
Sand Eel
Rockling
Whiting Pout
Mackerel
Herring
Crabs
* Oysters
* Muscles
Shrimps

There appears no reason why turtle should not also be cultivated, whether they would breed or not. The peacock,

pintado, pheasant, and common fowl, are the natives of hot climates, and have long been naturalized to cold ones; and there is far less difference between the temperatures of the water in different climates than between those of the air. An excellent turtle has been taken in the Tamar at Saltash, after an unknown length of residence.

ART. V.—*On the Mode of obtaining Silicium, and on the Characters and Properties of that Substance.* By M. BERZELIUS.* [*Phil. Mag.*]

Decomposition of Fluate of Silica by Potassium.

WHEN we read the description of the experiments made by MM. Gay-Lussac and Thenard on the silicated fluoric acid and potassium, we cannot doubt that they decomposed the acid in the circumstances which they describe. The potassium burns in the gas and condenses it; a brown matter is formed, which, boiled in water and dried, burns in oxygen gas with the evolution of silicated fluoric gas, and leaves as a residuum a white earthy matter. In applying myself to these researches, I looked upon the reduction of the fluoric acid and of the silica as so certain, that I thought it only necessary to examine the composition of the product obtained, in order to exhibit the subject in a clear light. The experiment of MM. Gay-Lussac and Thenard gave me the same results as they obtained, with this difference, that the mass when burnt in oxygen gas was not white, and that it had preserved its primitive colour without any remarkable change. I expected to find some fluate of silica and potash, but the concentrated sulphuric acid did not disengage any trace of fluate of silica; and although carried to ebullition, it did not produce any change in the substance. Nor was the substance attacked by any acid but the fluoric, which separated the silica, and left a deep brown matter, insoluble in acids, and which did not burn on exposure to the action of fire. Was this the radical of the fluoric acid, or that of silica, or a combination of both?

To procure this substance in greater quantity, I proceeded in the following manner; I introduced into a glass retort of the capacity of about ten cubic inches a small vessel of porcelain on which was a piece of potassium of the size of a

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* From the *Annales de Chimie et de Physique*, tom. xxvii. p. 337.

large nut. After having rapidly exhausted the retort, I let in some silicated fluoric gas, from a reservoir over mercury, and I applied the heat of a spirit-lamp. The potassium at first became white, but afterwards it became browner and browner, and at last as black as a coal: it was not long in becoming inflamed, and it burnt with a large flame of a deep-red colour, not however intensely; whilst the mercury rose rapidly in the reservoir, on account of the absorption of the silicated fluoric gas. As soon as the combustion ended, a vacuum was made in the apparatus, in order to prevent the formation of the fluate of silica and potash, and it was then left to cool. The product was a hard, porous, agglutinated, and deep-brown mass, which did not change in the air, but which like manganese, emitted the smell of hydrogen when it was pressed between the fingers or when breathed upon. In the retort, around the porcelain vessel, a light powder of a yellow-brown was remarked, which was preserved. The burn mass when thrown into water immediately produced a copious evolution of hydrogen gas. The water dissolved much fluate of potash, and the brown mass was precipitated in the form of a chestnut-brown powder, slowly evolving gas. The alkaline solution was descanted and replaced by pure water. The evolution of gas visibly diminished; and the water having again been renewed after some time, it entirely ceased; so that the brown powder did not decompose the water, even at the temperature of ebullition. The solution obtained by boiling it being very acid, it was boiled with fresh portions of water, until no signs of acidity were manifested, and the solution afterwards passed through the filtre. The water was saturated with fluate of silica and potash, and the brown powder was washed as long as the water left the slighest residuum by evaporation. The powder, being dried, was of a chestnut brown, and evidently contained heterogeneous parts of a brighter colour. The brown matter which was deposited on the glass during the combustion of the potassium was much more homogeneous. Water did not disengage hydrogen by its action, but quickly became acid. This powder was washed with the same care as the preceding.

Now, for the purpose of determining the change undergone by the brown matter during combustion, it was first perfectly dried and heated to a dull red in a current of hydrogen gas, and, after being weighed, exposed in a proper apparatus to a current of oxygen gas. As soon as the air of the vessel seemed to be displaced by oxygen gas, the brown matter

was heated with a spirit-lamp; it immediately took fire, and burnt vividly for some time, having a pale blue flame on its surface. The remaining oxygen being passed into barytic water, rendered it very turbid, and gave a precipitate which entirely dissolved with effervescence in muriatic acid, and which, consequently, was carbonate of barytes. The burnt mass was greatly diminished in bulk, but was of much the same colour as before. Its weight was augmented scarcely half a hundredth part. There was not perceived either in the glass balloon where the combustion took place, or in the glass conducting tube, the least indication of the presence of fluoric acid; and this showed plainly that this acid was not produced by the combustion of the brown matter; and that the presence of it in the experiments of MM. Gay-Lussac and Thenard, as also in the first which I made, arose from the circumstance that the brown matter contained fluate of silica and potash, which was decomposed by the heat of the combustion, and gave out silicated fluoric acid; this result likewise deprives us of the hope of learning by these means the composition of fluoric acid; but it is not less interesting, since it seems to show that the pulverulent brown matter is really *silicium*. Its combustion in oxygen, with the production of carbonic acid, without much increase of weight, was now not so difficult to understand, since it is a property of the oxides which contain three atoms of oxygen, that their quadri-carburet burns without increase of weight. But whence comes this carbon? how is it combined with the silicium? I thought at first that it might be attributed to the presence of the oil of naphtha in which the potassium was preserved, and I repeated the experiment with potassium which had not been in this oil. The result was the same. I then began to think that this metal might contain carbon; for it had been prepared after the advantageous process lately given by Brunner, which consists in distilling at a high temperature a mixture of carbonate of potash and of carbon in a vessel of forged iron. The potassium, distilled in a glass vessel, accordingly left a coally residuum, which, by treatment with water, yielded some potash and much carbon. In repeating the experiment of the combustion of potassium in silicated fluoric gas [employing the metal thus purified], the powder was not so brown, and when burnt in oxygen acquired an increase of 40 per cent without producing carbonic acid. Its colour, however, was much the same as before combustion. But this circumstance will no longer appear

astonishing, if we suppose, either that the silicium may take in its combustion a lower degree of oxidation, or that, as happens with boron, the silica which is formed prevents the entire combustion of the silicium. The residuum of the combustion, treated by fluoric acid, gave silicated fluoric gas, and its colour became much darker. Thrown on a filter, well washed and dried, it was pure *silicium*.

Description of Silicium, and of its Chemical Relations with other Bodies.

Obtained in the manner just related, silicium is of a dark nut-brown colour, without the least metallic lustre. When rubbed with steel it does not give any bright streak, and resists the friction as an earthy body does. It is incombustible in atmospheric air and in oxygen; it does not undergo any change in the flame of the blowpipe, and appears to belong to the class of the most infusible bodies. These properties seem opposed to what I have already said of the combustion of silicium, which is easily accomplished when we employ that substance as obtained immediately after its reduction by potassium. This difference in its combustibility is extremely remarkable. It does not depend on an effect attributable to the fluoric acid; for, if before burning the silicium it is treated with fluoric acid, a portion of silica will be extracted from it, which had been separated from the fluoric acid by the potash produced by the oxidation of the potassium by the air, before the experiment could be made; and the acid dissolves besides, especially by means of heat, a portion of silicum, with evolution of hydrogen gas. The silicium which then remains after filtering and washing inflames and burns with vivacity in air and in oxygen. Nor can this combustibility result from a residuum of potassium; for after combustion neither fluoric acid nor fluate of silica and potash can be extracted from the product; it appears to me to be owing more probably to a portion of hydrogen combined with the silicium; for, if the silicium be burnt in oxygen, even after having been heated in hydrogen or in a vacuum, water will be formed, but in very small quantity relatively to the great capacity of saturation of the silica. The silicium which is obtained when the brown mass procured with potassium is thrown into water is, after that, a *hydruret of silicium* or a *siliciuret of hydrogen;* the reduced mass is a *siliciuret of potassium*, which the water decomposes; the potassium is converted into potash

and dissolved; the greater part of the hydrogen is disengaged, and the smaller part takes the place of the potassium in combining with the silicium. If the hydruretted silicium is put into an open platinum crucible, and slowly heated until redness commences, and then after being covered carried to a white-red heat, the silicium loses its combustibility; and treated with fluoric acid, which now dissolves no more of it, becomes perfectly pure, without sustaining the great loss which takes place when we begin by burning it.—When the hydruretted silicium is very quickly brought to a red heat, it takes fire, because the hydrogen cannot burn at a lower temperature without inflaming the silicium at the same time. If the silicium does not burn completely in this case, that circumstance does not arise from the production of an inferior degree of oxidation, but from the access of oxygen being prevented by the silica formed. Besides the loss of hydrogen which the silicium sustains at an elevated temperature, it undergoes a further change; it loses the property of dissolving in fluoric acid, is contracted in bulk, and takes a darker colour. This circumstance has certainly as great an influence in diminishing its combustibility as the loss of its hydrogen. In the state of least condensation in which it is obtained, when just separated from the potassium by means of water, it may be compared, as to its combustibility, to the porous hydrogenated charcoal of lint, which takes fire by the sparks of a steel, but which loses this property after having been exposed to an elevated temperature. The incombustibility of silicium is besides so great, that the small quantity of it which remains on the filters may be found by burning them and treating the ashes with fluoric acid.

Silicium, even when dry, stains, and strongly adheres to the glass vessels in which it is kept. When it is treated with fluoric acid, the liquid is covered with a little pellicle, which envelopes, as fat oils do, each drop that is taken from it. This pellicle rises on the sides of the vessels so long as they are damp, and then appears, by refraction, of a clearer colour than the silicium which is under the liquid.

Silicium is a non-conductor of electricity. That which has become incombustible by its exposure to a strong heat does not undergo any change, if, whilst red-hot, chlorate of potash is thrown upon it. It is not attacked by saltpetre until the temperature is raised sufficiently to decompose the nitric acid, and the affinity of the alkali begins to act; but at a white heat it is acted upon with great energy by that salt.

With carbonate of potash, silicium burns very easily with a vivid inflammation; carbonic oxide is given out, and the reduced carbon imparts a black colour to the mass. The incandescence is so much the more intense, and the temperature requires to be so much less elevated in order to determine the action, as less carbonate of potash or of soda is taken. Thus, for example, taking a portion of carbonate equal in bulk to half the silicium, the inflammation takes place much below the temperature of ignition. With greater quantities of the carbonate, the mass becomes distended by the production of the carbonic oxide, takes fire and burns with a blue flame. If a still greater quantity of carbonate be employed, no signs of combustion can be perceived; the mass does not grow black, and merely gives out carbonic oxide.

From this mode of action of silicium with carbonate of potash arises a very paradoxical phenomenon. If some incombustible silicium is heated with saltpetre to a moderate red, on a leaf of platinum or in a little crucible, no action will be observed; but if there is added a little dry carbonate of soda, so that it may touch the silicium, a detonation will take place, at the expense of the carbonate, in the midst of the saltpetre, and the mass will preserve for some time its black colour.

The cause from which the silicium burns at a low temperature more easily with the carbonate than with saltpetre, is without doubt owing to the affinity of the alkali for silica being necessary to determine the combustion of the silicium, and not being able to manifest itself with the saltpetre except when the temperature is sufficiently elevated for the decomposition of the nitric acid. If the burnt mass continues black some time longer, this arises from the new combination being compact, and protecting the carbon until it enters into fusion.

Silicium detonates with the hydrates of the fixed alkalies, producing a vivid incandescence at a temperature at which they melt, but much below that of ignition. Hydrogen gas is given out, which is seen to burn when the bulk of the mass is not too small. The same phenomena are observed with the hydrate of barytes. Incandescence also takes place with the hydrate of lime; but it is very feeble, and the silicium is oxidated but imperfectly. With the acid fluate of potash it detonates at the fusing temperature of the salt, that is to say, much below a red heat; it is not affected by fused borax.

Silicium heated to a perfect red in the vapour of sulphur

inflames and burns, but with less intensity than in oxygen; and the combination even does not take place with the incombustible silicium. The sulphuration is usually as incomplete as the oxidation, and a scorified mass is obtained of a dark gray colour. It sometimes happens, however, particularly when a vacuum is made in the vessels before volatilizing the sulphur, that the silicium becomes completely sulphuretted, at least in a part of its mass. It then presents an earthy white body, which, thrown into water, instantly dissolves in it with the evolution of sulphuretted hydrogen. The silicium is converted into silica which dissolves in the water; and if this is in small quantity, a solution may be obtained so much concentrated, that it solidifies after a slight evaporation, and it leaves the silica, after the desiccation, in a transparent fissured mass. Silicium imperfectly sulphuretted also decomposes water rapidly, with the disengagement of hydrogen, and solution of the silica in the water; and the silicium which was not combined with the sulphur becomes separated. In the air the sulphuret of silicium diffuses a very strong smell of sulphuretted hydrogen, and loses in a little time all its sulphur; but in dry air it may be preserved a long time. At a red heat, it contracts and shrivels up, yielding sulphurous acid and silica. This change, however, takes place but slowly; for when kept at a red heat for some moments, it still has the property of decomposing water.

The siliciuret of potassium readily combines with sulphur at a red heat; but if the mass is dissolved in water, there remains much silicium, unless the mass be newly exposed to a white heat, because the silicium then combines with the sulphur at the expense of the potassium previously sulphuretted at a more elevated temperature. This combination is a true double sulphuret; its colour is of a dark brown, almost black. It presents a melted mass which dissolves in water. It is difficult to say whether it dissolves without changing its nature; but since the sulphuret of silicium is decomposed in water, and sulphuretted hydrogen has a great affinity for the sulphuret of potassium, it is very probable that in the solution there exists some silicate of potash with hydro-sulphuret of the same alkali. The combinations of the sulphuret of silicium with the metallic sulphurets, although they exist in the dry way, cannot, it appears, exist in water.

It is certainly a very remarkable fact to see the silica dissolve in so great quantity in water at the moment of its formation, and lose this property by evaporation, so as to become

insoluble in acids. This great solubility, shown by the preceding experiments, explains the numerous crystallizations of silica in the cavities of druses, which sometimes could not contain a volume of liquid ever so little greater than the crystal itself. Nevertheless, I do not pretend that the silica had been in solution as in our example; that is to say, that it was the result of the decomposition of the sulphuret of silicium.

I did not succeed in combining phosphorus with silicium by bringing it into contact with the latter at a red heat. I did not try any other process.

If silicium is heated in a current of chlorine, it takes fire and continues to burn. If the gas contains atmospheric air, some silica remains in the form a slight skeleton. Silicium burns equally well in chlorine, whether or not it has lost its combustibility in the air. The product condenses and presents a yellowish liquid when it contains an excess of chlorine, but which is without colour when it is freed from this excess. This liquid is very fluid; it evaporates almost instantaneously when exposed to the air, yielding white vapours, and leaving a residuum of silica. It has a very penetrating smell, which may be compared in some degree to that of cyanogen. Thrown into water, it floats on the top; it generally dissolves in it, or leaves a little silica. If the quantity of the water is small, a drop, for example, on as much choride of silicium, this envelopes it, and the silica remains in a frothy semi-transparent mass. This liquid is analogous to the combinations of the other electro-negative bodies with chlorine. It reddens litmus paper; it is the second example known of a liquid combination formed by silicium.

At common temperatures potassium has no action on chloride of silicium; but when heated in the vapour of the latter substance it takes fire and produces a compound of potassium and of silicium. The iodide of potassium does not combine with silicium.

Silicium is neither dissolved nor attacked by the sulphuric, nitric, or muriatic acids, and not even by aqua regia. In its combustible state it is slowly dissolved by fluoric acid with the evolution of hydrogen; but in losing its combustibility, it loses also the property of dissolving in this acid. It is, on the contrary, dissolved with rapidity, even in the cold, by a mixture of the nitric and fluoric acids, nitrous gas being given out. Combustible silicium is dissolved by digestion in a solution of caustic potash; but when rendered incombustible, it is no longer attacked by the alkalies in the humid way.

Silicium, when once isolated, combines with the metals with much difficulty. Its remarkable affinity for platinum is known by the experiments of M. Boussingault; but it may be heated as often and as long as we choose in a platinum crucible, without any combination taking place. But if we endeavour to reduce silicium by potassium in a platinum crucible, the silicium penetrates deeply into the platinum wherever this is touched by the potassium. Copper, silver, lead, and tin, heated with silicium by the blowpipe, do not seem changed in their appearance, nor in their ductility; notwithstanding when they are treated with acids they leave a small quantity of silica. The copper, particularly, left a skeleton of the same form as its own. It is here to be remarked that silicium, which alone is not attacked by the acids, is oxidated by them when its combinations with the metals are dissolved. We have, however, a similar example already in rhodium, which, though not attacked *per se* by aqua-regia, is dissolved by it when alloyed with certain metals.

Titanium, which approaches so near to silicium in its properties, is also insoluble in acids in the metallic state (with the exception of a mixture of the fluoric and nitric acids), whilst it is oxidated and easily dissolved when alloyed with other metals.

Silicium combines with potassium at a high temperature, but without the evolution of a remarkable heat. It affords two combinations; one, with excess of potassium, of a dark-gray, and which dissolves completely in water; the other, with less potassium, is obtained by the reduction before stated, or by exposing the first to a very strong heat. It is besides probable that silicium may form with the metals combinations, the proportions of which correspond with those of their silicates.—I reserve these researches for another time.

[*To be continued.*]

ART. V.—*On a Scarlet Sub-chromate of Lead, and its Application to Painting and Calico Printing.* By JOHN BADAMS, Esq. [*Ann. Philos.*]

IT was announced by Dulong, in the Ann. de Chim., so long ago as the year 1812, that by boiling together carbonate of lead and chromate of potash in excess, a red sub-chromate of lead is produced, containing exactly double the quantity of lead in the common chromate. Ten years afterwards, in the

same journal, Grouvelle announced the existence of a red chromate, and gave several processes for preparing it, but made no mention of Dulong's discovery. It is remarkable that neither of these chemists, nor any subsequent writer, appears to have noticed the important uses to which this substance is applicable.

Having made a variety of experiments upon its properties, as a fast colour in calico-printing, and a durable pigment for artists in oil and water, I wish to give publicity to the advantages which, I think, the arts will derive from its adoption.

Grouvelle's method of preparing red chromate of lead consists in boiling the common yellow chromate with potash; for practical purposes, this is much more convenient than Dulong's process, and gives a very fine colour. By some unaccountable blunder, however, the ingenious author of this formula so completely mistakes the nature of the substance produced by it, that every step he takes in his analysis to explain the composition of the red chromate, only tends to involve it in deeper obscurity. But he must speak for himself.

"J'ai analysé comparativement le chromate jaune, le rouge, et le plomb rouge de Sibérie. Tous donnent *exactement* le même rapport entre *l'acide* et *l'oxide*. Ce sont des chromates neutres; seulement le chromate rouge contient une petite qauntité d'alcali, qui m'a paru etre de là 1. 5;" and yet in the very next page, the author contradicts himself, and says, in summing up,

"Il suit de là que l'alcali parait être combiné à l'oxide de plomb, et que cette combinaison unie au chromate de plomb, donne lieu au chromate rouge, qui contient ainsi un peu *plus d'oxide* de plomb que le chromate neutre."

Grouvelle then goes on to assert, that he finds little alkali (lime) in the native red lead of Siberia, though, as he admits, this very ore becomes yellow on being powdered, which, if it really contained lime, it ought not to do.

I shall now endeavour to show that the whole of this analysis is erroneous.

(A.) 100 grains of scarlet chromate of lead made by Grouvelle's process, were digested for half an hour with constant stirring in very diluted acetic acid. The red colour gradually but totally disappeared, and was succeeded by a pure yellow. This yellow powder, carefully washed and dried at 300°, weighed 60 grains, and proved on examination to be common chromate of lead.

Here then we have red chromate of lead (said by Grouvelle

to contain exactly the same proportions of oxide and acid as the yellow chromate) resolved into 60 grains of yellow chromate and 40 grains of some other substance, which we shall see, by the next experiment, to be oxide of lead.

(B.) The acetic acid, holding in solution 40 grains of the original weight of the red chromate employed, was perfectly colourless and transparent; and on being gently evaporated in a water bath, shot entirely into crystals, bearing the well-known character of acetate of lead. On redissolving these crystals in water, and boiling them with an excess of carbonate of ammonia, a dense white precipitate fell, weighing, when dried, 46 grains=38.45 nearly oxide of lead.*

(C.) To ascertain whether any sensible portion of potash originally entered into the composition of this red chromate, the ammoniacal solution was evaporated gently in a large platina crucible, and gradually exposed to a red heat; the whole residue sublimed, leaving only a scarcely perceptible stain of lead upon the surface of the crucible. A little water now boiled in the same crucible did not affect the colour of turmeric paper in the slightest degree; hence no potash whatever can be suspected.

On repeating these experiments, I feel satisfied that the 40 grains of loss sustained by the red chromate in experiment (A), were entirely due to oxide of lead; and that 100 grains of red chromate were resolved into 60 grains of yellow chromate and 40 oxide of lead. But 60 grains of yellow chromate are composed of 40.98 oxide of lead, and 19.02 chromic acid; hence it cannot be doubted that yellow chromate contains just half the proportion of oxide in the red chromate; in which case the atomic constitution of the two chromates of lead will be as follows:

Yellow Chromate.	*Red Chromate.*
Chromic acid....19.02, 1 atom	Chromic acid...19.02, 1 atom
Oxide of lead....40.98, 1 atom	Oxide of lead...81.96, 2 atoms
Or 100 parts consist of	
Chromic acid.....31.7	Chromic acid.....18.84
Oxide of lead.....68.3	Oxide of lead.....81.16
100.00	100.00

* The difference between 38.45 and 40 is owing to the imperfect precipitation of oxide of lead by carbonate of ammonia.

In order further to show the true composition of red sub-chromate of lead, as I may now venture to call it, I took 60 grs of yellow chromate and ground them with 40 grains of oxide of lead, adding small quantities of hot water from time to time: they united into a red sub-chromate.

Uses.

I have endeavoured, at some length, to clear up the theory of the two chromates of lead, because one radical error of the nature which Grouvelle has committed, leads to endless mistakes and disappointments in the practical operations which are founded upon it. I have before alluded to the advantageous use of sub-chromate of lead in giving a permanent orange scarlet upon cotton. The mode of its application to calico-printing will be obvious to all who know the principle upon which yellow chromate of lead is made fast; and no intelligent calico-printer will need any further information from me, as his own practice in fixing the yellow chromate, added to what has been here said of the nature and preparation of the red sub-chromate, will readily suggest what is necessary. I shall only remark that nitrate of lead and an alkaline solution of chromate of potash, will give him the colour. He may accumulate, besides, some insoluble salts of lead in the pores of the cloth, to give stability to the tint, and may modify the operation by various means, but in all cases the colours must be heightened at last, by passing them through boiling water.

Scarlet sub-chromate of lead is extremely beautiful when ground up with oil, and possesses great *body* as a pigment. It is not degraded in its hue, like vermillion, by admixture with white lead; it mingles with other colours, and shows no signs, after a long exposure, of any change by time. As a water-colour, I dare not yet say that it has been tried sufficiently to authorize a positive declaration that it will not blacken, but several pieces of cards and thin paper painted with it, and hung upon the walls of inhabited houses, likely to influence the colour of salts of lead, have not, in some months, perceptibly diminished in brightness. Should a longer experience confirm the promise already given, I shall be happy to make it known to artists, as they have nothing equal in colour to red lead which they can trust in their drawings for a month even, and no tint would be a more desirable accession to the pallet, than a bright and permanent scarlet, or scarlet orange.

ART. VII.—*Essai géognostique sur le gisement des Róchés dans les deux Hémisphères, par A. de Humboldt.* 1 vol. 8vo. Paris, 1823.——*A Geognostical Essay on the Superposition of Rocks in both Hemispheres.* Translated from the Original French. London, 1823. [*Jour. Roy. Inst.*]

IT is known that Baron Alexander Humboldt is a great traveller, and it is believed that he is a great Astronomer, a great Botanist, a great Political Economist, and to sum up all, a great Philosopher. It remained to be proved that he was also the great Geologist that he had been believed; and the proof is, the book before us.

The Title, implicating the whole sphere, (which is obviously produced by the addition together of two hemispheres,) would have led us to expect the history of all the rocks of our sphere, or globe, had we not been accustomed to the grandiloquism of our author; and we ought, therefore, to inform our readers, that the "gisements" in question are confined to a very small portion of Europe, and a much smaller one of America. The other two hemispheres, Asia and Africa, will probably appear in a future volume. But this work embraces "pour ainsi dire, toute la geognosie positive;" a fact which we are very happy to hear, as the shelves of the Geological Society may now discard the lumber, German, French, and Italian, besides English, with which they are loaded, and replace it with this volume of quintessence.

Before proceeding further, however, we ought probably to apologize to Baron Humboldt, to our readers, and to what is commonly called the world, for daring to take liberties with a Great Man, or a Great Name rather. It is the courtesy of society to fall down and worship the idol which it has set up, and thus that Scotch Entity, called the Great Unknown, is permitted to write with impunity even *St Ronan's Well.* All that we can say, however, in our defence is, that whatever respect we may have for Trojan or Tyrian, we have more for truth; and that if Plato himself had written on the two hemispheres, we should have equally asked, what he knew of them.

Why will not M. Humboldt tell a plain tale plainly; if he had useful geological information to give on an unknown country, he might have done it without all this *pretension.* His book is not a system of Geognosy, and it is not a description of the positions of the rocks in the two hemispheres. The Equator, the Andes, the Amazon, the Oroonoco, are sono-

rous words, but the world already knows, and is wearied of hearing, that he has travelled them; weary of his travels, weary of his latitudes, weary of his isothermal lines, weary of his geography of plants and animals, and weary of him. The very Faubourg St Germain, the saloons, the coteries, are as weary of the Andes, as are Cuvier, Arago, and Fourrier. We must not, however, transgress the circle here drawn round us; yet, in this narrow book, we can find matter for caution and advice, which in sincerity and friendship, whatever M. Humboldt may think, we offer to him. He has seen much that others have not; he has collected facts that others have not; he has travelled, and he might have instructed; but not content with such fame, and such praise as he might have commanded, he has been resolved to be a philosopher and a Sublime; to Chateaubriandize on weeds, and algebraize on stones, forgetting that he who has placed one foot on the pinnacle is thus much nearer to the bottom—"il n'y a qu'un pas."

There is a certain singleness and simplicity of apprehension necessary to writing clearly, and this is a quality, we grieve to say, which M. Humboldt does not possess. If there be an idea, it is involved in such a mass of words that it is suffocated; if there be two, they become so entangled that their separate identity disappears. The Baron's writing appears to have set for its portrait to M. Fuselis' lectures on painting. The purpose of language, says Swift, (among others) is to conceal your thoughts; but it is another important purpose, which he has overlooked, to be a substitute for them.

There are always two terms, a good and a bad one, for every human action, for every thing. Michael Angelo is grand, or extravagant and absurd. M. Humboldt professes to generalize; his views are grand, broad, poetical; or else they are fictitious, slovenly, or dreamy. The mob has voted "the ayes;" we are of the contrary opinion. In philosophy, in science, natural or otherwise, we know of no generalization that is not a collation and abstract of facts. Without this, it is all guess and fancy; and the fancies are commonly abundantly dull, moreover. Isothermal curves may captivate the uninquiring herd, as do the crotchets of algebra, since they look profound, like lord Burleigh; but like lord Burleigh, it is to say nothing. We should be much more grateful to M. Humboldt if he would particularize; we do not want pages of guessing; and if we are to read poetry, let it be John Gilpin or Childe Harold; nor "Goddess write about it and about it."

Can we now convince M. Humboldt that we are his friends and well wishers? if we tell him of his faults, where else will he find sincerer ones? But we must proceed to the "Gisement des Rôches."

Upwards of 80 pages are occupied in something that looks like an introduction, being a loose collection of facts, words, and opinions; and this is followed by an account of all the acknowledged rocks, from granite upwards. Such is the plan of the work, which is nearly as undigested as its materials are indigestible.

To give any thing like an analysis of the introductory matter, which seems intended as a sort of abstract of the science, is impossible. We are lost amid the vagueness of what might have been compressed into half the space, or less, while it ought to have been divided into paragraphs or subjects. It is not thus that philosophy is to be written or taught; and if this be Geognosy, we shall remain content with simple Geology. If there is a want of precision, of *definiteness* in the manner, there is not less in the matter; while with a mixture of pedantry and modernism, there is that antiquated phraseology, derived from Freyberg, which shows that the author has not reached up to the level of our present knowledge, but is merely ingrafting a few facts from a better source, on the cant and dogmas of his school. The want of purpose, the vagueness of ideas, the parade of science, which are so characteristic of this writer, will perhaps be rendered as visible by the following passage as by any other; and it is really selected without any invidious motives, and as an example of his style and turn of thinking

"We must not confound (a point on which I have often insisted in this essay) rocks passing insensibly to those with which they are in immediate contact; for instance, mica-slates that *oscillate* between gneiss and clay-slate, with rocks that alternate together, and preserve all their distinct characters of composition and structure. M. d'Aubuisson has long since shown, that chemical analysis connects clay-slate with mica. (*Journal de Physique*, tom. lxviii. page 128. *Traité de Géognosie*, tom ii. page 97.) The former it is true has not, indeed, the metallic lustre of mica-slate: it contains a little less potash and more carbon; the silica does not unite in knots or thin plates of quartz, as in mica-slate; but there can be no doubt that the scales of mica constitute the principal basis of clay-slate. These scales are so united together, that the eye cannot distinguish them in the mass. Perhaps this very affinity prevents the alternation of clay-slate and mica-slate; for in

these alternations nature seems to favour the association of heterogeneous rocks, or, to use a figurative expression, she delights in those associations in which alternating rocks exhibit a great contrast of crystallization, mixture, and colour. I saw in New Spain dark-coloured greenstone alternating thousands of times with reddish-white syenites, that abound more in quartz than in feldspar; veins of syenite occur in this greenstone, and veins of geenstone in the syenite; but neither of those rocks passes into the other. (*Essai Politique sur la Nouvelle Espagne*, tom. ii. p. 523.) At their actual contact, they exhibit differences as strongly marked as the porphyries that alternate with grauwackès and syenites, the black limestone that alternates with transition clay-slate, and many other rocks altogether heterogeneous in their composition and general aspect. It may be observed further, that when in primitive formations, rocks more nearly allied to each other from their composition than by their structure, or their mode of aggregation, for instance, granite and gneiss, or gneiss and mica-slate, alternate, those rocks do not show the same tendency to pass into each other, which they display in non-complex formations. We have already observed above, that a bed β, when it occurs more frequently in the rock α, points out to the geognostic traveller, that the simple formation α is about to be succeeded by a complex formation, in which α and β alternate. Afterwards it may happen that β assumes a greater developement; that α is no longer an alternating rock, but merely a subordinate bed to β, and that this rock β appears by itself, till by the frequent appearance of beds γ, it forms the prelude to a complex formation of β alternating with γ. We may substitute to these signs the words granite, gneiss, and mica-slate; those of porphyry, grauwackè, and syenite; of gypsum, marl, and fetid limestone (*stinkstein*); but the *pasigraphic* language has the advantage of generalizing problems, and conforms more to the requisites of *geognostic philosophy*, of which I here attempt to trace the first elements, as far as regards the study of the superposition of rocks. But, between formations, simple and nearly connected in the order of their relative antiquity, between the formations $\alpha\,\beta\,\gamma$, complex formations, $\alpha\,\beta$ and $\beta\,\gamma$, are often found placed, (that is, α alternating with β, and β alternating with γ;) it is also observed, though less frequently, that one of those formations (for instance, α) takes an increase so extraordinary, that it envelopes the formation β, and that β, instead of appearing as an independent rock, placed between α and γ, is only a bed in α. Thus, in Lower Silesia, the red sandstone contains the formation of zechstein; the limestone of Kunzerdorf with impressions of fish, and analogous to the bituminous marl of Thuringia abounding also in fish is entirely enveloped in the coal sandstone. (Buch. *Beob.* t. i. p. 104, 157; Id. *Reisenach Norwegan*, t. i. p.

158; Raumer, *Gebirge von Nieder-Schlesien*, p. 79.) M. Beudant (See *Min.* t. iii. p. 183,) has observed a similar phenomenon in Hungary. In other regions, for instance, in Switzerland, and at the southern extremity of Saxony. the red sandstone disappears altogether; being replaced, or in some sort suppressed, by an immense developement of grauwackè, or of alpine limestone. (Freiesleben, *Kupfersch*, b. iv. 109.) These effects of the alternation and unequal developement of rocks are so much the more worthy of attention, as the study of them may throw light on some apparent deviations from a type of superposition generally received, and may serve to refer to a common type the series of positions observed in countries very distant from each other.

We might make many commentaries on this extract, but shall content ourselves with few. We do not see any necessity for "insisting" on our not confounding transitions of rocks into each other with mere alternations; and cannot perceive the vast value of the caution, nor the importance of the enunciation, as if it were the author's own discovery. As to M. d'Aubuisson, with his mica and thonschiefer, he is worthy of Humboldt. We may ask either, or both, what on earth the chemical analysis of a mixed rock proves, and what important consequences follow from knowing that mica and clay-slate contain the same earths. The analysis of a hundred sandstones would correspond with that of as many granites; hornblende contains the same earths as clay-slate, and in the same proportion; so does shale, so does clay itself, so does basalt, so does greenstone, and so does pitchstone. We may add, with Voltaire's "Nonchalant," what then? As to our "not doubting that scales of mica constitute the principal basis of clay-slate," we certainly not only doubt, but deny it. The one is a crystal, or a crystallized mineral, the other not; and if we were to seek for an identity "not to be doubted," it would rather be between clay and clay-slate.

That nature "favours the association of heterogeneous rocks," is one of those frivolous and false generalizations which characterize the whole of this author's writings. It is not the fact, and if it were, what then? If there be great spaces or depths of one rock without intermixture, why then there are; and if there are not, why then nature must "please herself" with differences. These are words; and thus it is with this author, as if a different wordage was philosophy and a discovery, a generalization and *une belle idée*.

There is another of these general laws equally unmeaning, and, to say the truth, not exactly intelligible, at least in Eng-

lish, whatever it may be in French or German geognosy, or in the geognosy of New Spain. It is that "when in the primitive class, rocks more nearly allied to each other from their composition than by their structure or mode of aggregation, such as granite and gneiss, or gneiss and mica-slate, alternate;" "those rocks do not show the same tendency to pass into each other, which they display in non-complex formations." It is not by discovering such laws as this that geognosy or any gnosy will ever become a science.

Here also we stumble on the threshold of the author's new geognostic algebra; and unquestionably, there is much profundity in a pasigraphy of alpha, beta, gamma, delta; but as we find that this new lithological algorithm is lying in wait for us again in the appendix, we shall for the present pass it over, as well as the remainder of a paragraph, which, thus far, we owed to our readers.

There are many ways of writing a new book, or a new paragraph, or of seeming to have discovered a new idea, or to have made a discovery; and there is none much more effective than that of translating a plain, vulgar, intelligible, well-known fact or proposition, into long-winded, altisonant, and obscure phraseology. For example, it does not follow that, because any given limestones, such as that which lies beneath the red marl, contains pectines or terebratulæ in England or Greenland, it must also contain them in Peru or Napaul, and it is not true that because an echinus belongs to chalk, it must not also exist in slate. Nothing can be plainer; the truth is, that it is a great deal too plain. Now mark how it is improved by dress, how it is adornized, till we are as puzzled to recognise it as we should be to know Mr Waithman, when tricked up in all the robings and state of a lord Mayor.

"In the case of the conformity of position, there may be identity of mass (that is, of mineralogical composition,) and of diversity of the fossils or diversity of mass and identity of fossils. The rocks β and β, placed at great horizontal distances, between two identical formations, α and γ, belong either to the same formation, or are parallel formations. In the first case, their composition is similar; but on account of the distance of places, and effects of climate, the organic remains which they contain may differ considerably. In the second case the mineralogical composition is different, but the organic remains may be analogous. I consider the words *identical formations*, *parallel formations*, as indicating the conformity or non-conformity of mineralogical composition; but they do not enable us to prejudge respecting the identity of fossils. If it be sufficiently

probable that the deposits β and β. placed at great horizontal distances between the same rocks, α and γ, are formed at the same epochs, because they contain the same fossils, and are of the same composition, it is not equally probable that the *epochas of formation* are very distant from each other, when the fossils are distinct. It may be conceived that in the same zone, in a country of small extent, generations of animals have succeeded each other, and have characterized, as it were by particular types, the *epochs* of formations; but at great horizontal distances, beings of various forms may have occupied simultaneously, in different climates, the surface of the globe, or the basin of the sea. It may be observed further, that the position of β between α and γ proves that the formation of β is anterior to that of γ, and posterior to that of α; but nothing gives us an absolute measure of the intervals of time (*les époques-limites*); and different insulated deposits of β may not be simultaneous."

The man who, like Mr Hazlitt, proposes to cover a certain surface of paper with a certain number of words, would be very silly if he were to trouble himself about ideas, propositions, or arguments. The public expects nothing but a certain jack-o'-lantern flitting and dancing, which amuses the eye without encumbring the thinking powers; and it is satisfied. It reads, because it is necessary or convenient to read; and the repose of its faculties remains undisturbed. And this is very charming reading too; as they will vouch, who have scanned, with never exhausted delight, the exquisite verses of a "Person of Quality," which the prophetic mind of Pope sent forth, a model to succeeding ages. But he who, like Eudoxus or Euclid, deigns to assert some fact, or to give reasons why a fact should be believed, or to do both the one and the other, has generally been desired to tell the nature of his proposition and to assign his reasons. The very tailor himself, is indeed unreasonable enough to expect something of this kind, when he is to make a coat, and when it is to be, or not to be, made with a Brummel collar, or a Petersham pocket. In all the sciences, from piquet to political economy, as Hoyle and Adam Smith have shewn, and even in geology or geognosy, those who either understand, or desire that others should understand, usually state propositions and subjoin reasons; and, most commonly, when a man writes a paragraph, it is a paragraph, and nothing else. It contains a head, a body, and a tail, much in the manner of a horse, or of one of Cicero's orations; and does not run out into protuberances, or comprise various heads and tails, or, beginning with the history of a cock, terminate with that of a bull.

The figure called rigmarole has not generally been held in very particular esteem in the mathematical sciences. It is lucky, however, that it is approved of by the geognosts; and really there is such an affinity, and a harmony, between the two categories of geognosy and rigmarole, that we begin to think ourselves in error in doubting the utility and adaptation of the manner before us to the matter. We were about to have said, "De grace, Monsieur le Baron, humanisez votre discours et parlez pour être êntendu." But to what purpose; when there is nothing to be understood?

We have heard of a man running away with an idea, and we have also heard of an idea running away with a man. We have no great objection to either. But when a man is "run away with" by half a dozen at a time, his sentences and paragraphs are apt to become as disjointed as he himself would be, had he been run away with by as many wild horses. There is a vulgar maxim, called sticking to the point, which has been sometimes recommended, and which might be advantageously applied somewhat more widely, as for example:—

"From these general considerations on zoologic characters, and the study of fossil bodies, it follows, that notwithstanding the admirable labours of Camper, Blumenbach, and Sömmering, the exact determination of species, and the examination of their relations with beds very recent and close to the chalk, does not date farther back than twenty-five years. I believe that the study of fossil organic bodies, applied to all other secondary and intermediate beds by geognosts, who consult at the same time the position and mineral composition of rocks, far from overthrowing the whole system of formations already established, will rather serve to support, improve, and complete its vast series. The geognostic science of formations may, no doubt, be investigated (*envisager*,) under very different points of view, according as we give a preference to the superposition of mineral masses, to their composition (that is, their chemical and mechanical analysis,) or to the fossils which are contained in many of those masses; the whole of these are included in the science. The denominations *geognosy of position*, or of *superposition*, *oryctognostic geognosy* (which considers the texture of the masses), and *geognosy of fossils*, designate, not branches of the same science, but various classes of relations which it is necessary to insulate, in order to study them more particularly. This unity of the science, and the vast field it comprehends, were well recognised by Werner, the founder of positive geognosy. Although he did not possess the necessary means for attaining a vigorous determination of fossil

species, he never failed, in his course of lectures, to fix the attention of his pupils on the relations that exist between certain fossils and formations of different ages. I witnessed the high satisfaction which he felt, when M. de Schlottheim, one of the most distinguished geognosts of the school of Freiberg, began in 1792 to make those relations the principal object of his studies. Positive geognosy has been enriched by all the discoveries that have been made on the mineral constitution of the globe, and furnishes valuable materials to another science, improperly called the *theory of the earth,* which comprehends the first history of the catastrophes of our planet. It reflects more light on that science than it receives in its turn; and without contesting the ancient fluidity or the softness of the stony beds, (a phenomenon proved by the fossil bodies, by the crystalline aspect of the masses, by the rolled pebbles, or the fragments imbedded in the transition and secondary rocks,) positive geognosy does not pronounce on the nature of the liquids in which it is said that the deposits were formed, those *waters of granite, porphyry,* and *gypsum,* which in hypothetic geology, are made to arrive tide by tide on the same point of the globe.

For one paragraph, this is somewhat lengthy, as Jonathan might say. What the proposition may be, is another question. Is it fossil remains, or a method how to "envisager" geognosy, or to "envisager" the merits of Werner, or about a theory of the earth which is not a theory, or to prove that though Werner is the greatest geognost that ever lived, he had no business to make "those *waters of granite, porphyry,* and *gypsum,*" arrive, "tide by tide sur un même point du globe?" When a man sits down to write a paragraph, he would find it convenient to ascertain what he means to say. He should take a long breath, also, lest he run himself out of wind; an accident which we have observed to happen, in conversation, to those who deal in these same interminable paragraphs.

As to what may be the main matter, or rather one of the main matters of the aforesaid paragraph, we really have some doubts respecting the creative powers of Werner in positive geognosy. The positive quality we grant, as this geognosy never betrayed any want of confidence and assertion. The remainder might be mooted if we were much inclined that way. We remember the late Mr Tennant, and we remember his pronouncing in one of his lectures respecting this creator of positive geognosy,—"a stupid German blockhead;"—"aside," however, as is sometimes the usage of the stage. Really, Mr Tennant, these were hard words;

but Mr Tennant had not scaled Cotopaxi or Popocatepetl.—Dans les pays des aveugles les borgnes sont Rois.

There is a vast virtue in hard words, just as there is in the *x*'s and *y*'s of Algebra. Baron Humboldt is not unacquainted with the value of "Loxodromisme," however we may be. The office near Soho-square for plundering, alternately, servants and masters, would lose half its attractions but for its Therapolegia inscription. The guinea paid for supplying it was well bestowed. Loxodromism or Loxodromy. What a mass of knowledge is contained in this little word. Elevation, parallel position, these are household terms, and carry no weight. The clown does not admire the preacher whom he understands; "he is no Latiner." When a man can express himself in the ordinary language which he has picked up from his nurse, we have generally a suspicion that he knows what he means. Solomon seemed to be of the same opinion, when he complained of the man who "darkened counsel by words without knowledge."

This Greek coinage is a false money, which cheats us with a semblance for solid gold. We wish the French nation would burn its Lexicons, since it has not yet learnt to make any other use of them. Great evils arise from petty causes, now and then. Had Brongniart not been the lucky (unlucky for us) possessor of a Greek lexicon, we should have been spared Phyllade, and Dolerite, and Trachyte, and Psammite, and Euphotide, and Diabase, and heaven knows what more; we should have been spared the arrangements, and the system, and the whole, and we might now have understood what his nation is writing about, if indeed they understood it themselves.

But the Greek is not the sole criminal, for the German does as well. Geognosy, or aught else, there is nothing so easy as phrases, phraseology, conventional language; cant, we were very nearly saying. All the Gnosies have their phrases and their cant; and words go for sense. They save the trouble of thinking, which is a vast convenience; and of understanding, which is more convenient still. Explain, explain, as they say in the House of Commons. Read Swift, and learn to write your own language; read Blaise Pascal, Mons. le Baron.

But we must return to the Loxodromy, the oblique course of the strata; and of Mr Humboldt's ideas. And now we shall see the reasons which induced the GREAT TRAVELLER to travel to Pichinca and Portocabello.

"Since the year 1792 I have been attentive to this parallelism, or rather to this *loxodromism* of beds. Residing on mountains of stratified rocks, where this phenomenon is constant, examining the direction and dip of primitive and transition beds, from the coast of Genoa across the chain of the Bochetta, the plains of Lombardy, the Alps of St Gothard, the table-land of Swabia, the mountains of Baireuth, and the plains of Northern Germany, I have been struck, if not with the constancy, at least with the extreme frequency of the directions nor. 3-4ths of the compass of Freiberg (from south-west to north-east). This inquiry, which I thought would lead naturalists to the discovery of a great law of nature, at that time interested me so much, that it became one of the principal reasons for my voyage to the equator. When I arrived on the coast of Venezuela, and passed over the lofty littoral chain and the mountains of granite-gneiss that stretch from the Lower Oroonoko to the basin of the Rio Negro and the Amazon, I recognised again the most surprising parallelism in the direction of beds; that direction was still nor. 3-4ths (or N. 45° E.), perhaps, because the littoral chain of Venezuela is not far from the angle which the central chain of Europe forms with the meridian. I announced the first results of observations which I made on the primitive and transition rocks of South America, in a Memoir published by M. Lamétherie, in his *Journal de Physique*, tom. liv. p. 46. I had mixed (as is often the case with travellers, who publish the results of their labours during the course of their journeys,) some very precise observations on the great uniformity in the direction of beds, (at the Isthmus of Araya, the Silla of Caracas, at Cambury near Portocabello, on the banks of the Cassiquiare: see my *Relat. Hist.*, tom. i. p. 393. 542. 564. 578., tom. ii. p. 81. 99. 125. 141.) with some general views which I have since regarded as vague and less accurate. Four years of travelling in the Cordilleras rectified my ideas on a phenomenon far more important than was hitherto believed; and at my return to Europe, I hastened to publish the general result of my observations in the *Geographie des Plantes*, p. 116.; and in the *Essai Politique sur la Nouvelle Espagne*, tom. ii. 520. Those results remained, no doubt, unknown to the learned author of the *Critical Examination of Geology*, p. 276., where he combatted the assertions published during my absence, in 1799, by M. de Lamétherie."

Now does Mr Humboldt really conceive and assert that he was the discoverer of the continuous and orderly elevations of strata, that this was an object to run away to the equator about, and need he tell us, for the twentieth time, that he is *The Traveller* of New Spain, that he has published a Geographie des Plantes, an Essai Politique sur la Nouvelle Espagne,

a Relation Historique, and a book of Personal Narrative? As to the "importance" of the fact, he surely does not imagine that it was pointed out by him; and as to his ideas having been rectified by four years of running about the Cordilleras, we only grieve that he did not continue his "courses" for forty, that they might have undergone a general rectification.

We have no dislike to M. Humboldt. On the contrary, we esteem him as an industrious and amiable man, ambitious, busy, and good-humoured, really, qualified, in many very important matters, for a general traveller. But his ambition has been of too vaulting a nature, and he has overleaped the point of success and security. He would write of every thing, and on every thing, he has been vague, inaccurate, wordy, and wearisome. He would be the name to fill all Europe with its sound, and he has, for this purpose, filled it with his books. He has blown the trumpet for himself, and he has found friends to blow it for him. There is no base for all this building. It is too much the custom of the day to work up to some niche in the temple of Fame, by contraband procedure, and it is not for M. Humboldt's sake alone that we make these remarks

Public justice demands that all should have equal justice. Of fame, there can never be but a certain portion in the world; for, where all are rich, no one is wealthy. If it is to be monopolized, let it be alloted where it is merited, for, when it is alloted otherwise, the meritorious must be robbed of their share and their rights. The Journalist and Reviewer is the guardian of public justice in Literature and Science; and it is his duty, if often neglected, to see that praise is duly and justly alloted, that he may thus protect the feeble, or the neglected, whom the public will not protect, and who cannot protect themselves. That public will not listen to him, or about him, who is not a Humboldt (as the phrase is,) or a something else; but it never stays to inquire what a Humboldt is, or what a Humboldt has really done. For the sake of the injured and the oppressed, for the sake of civil justice, we must strip off all that is fallacious or borrowed; we might do it for the purpose of criminal justice, as is the fashion with modern criticism, that we might punish him who deserves punishment. But this is not our object; and, from this intention, we desire that the subject of our remarks will exempt us.

We are not criticising the total works of this author, but we may say that he who has not simply been ambitious to

shine alike as a general traveller, as an astronomer, a botanist, a geologist, a politician, and much more, but who has laboured, directly and indirectly, to make the world believe all this, and who has contrived to succeed, ought to have been better informed, more accurate, and more dependant on his own powers and resources. Had he been all this, indeed, we should have heard far less of him, for he would have been more modest, and modesty is not now the purchase money of fame. There may be an excuse for bad writing, and confusion of thought and repetition, because an author cannot go beyond his powers. But there is little excuse for the perpetual ambition of being every thing; and there is none for inaccuracy, where accuracy is asserted, and where it is easily attained, nor for claims to originality founded on the labours of others. He at least who carries quadrants and wearies us with angles and immersions, is bound to be correct in longitudes, and most certainly in latitudes; and he who publishes the statistics of New Spain, would have judged more wisely had he informed us that they were a collection of public documents, and not the proper result of his own labour and observation.

But we must pass on to the work before us, and to the geology, or geognosy of M. Humboldt. What we have extracted, will serve to show the laxity and vagueness of this author's ideas, no less than his writings. And it is not for the poor purpose of criticising him alone, that we have examined his book; but because, through him, we hope to induce other writers on the same subject, to introduce more precision and purpose into their geognostical writings. We are already encumbered with a mass of useless and unintelligible matter, fit only for bonfires, and if we are to go on thus, a library of geology will soon rival the libraries of physicians. This is an age in which every man can write something, and in which every man thinks it necessary to write a book on something. Geognosy has the especial merit of being an easy subject, "heaven bless the mark," since by means of Hungary, Saxony, flötz, transition, and dolerite, with a little aid from loxodromism, hyenas, potamoid, acephalous, types, and a few more magic words, volumes are made, as volumes have been made before. Some mercy is due to the unhappy student, at least; and he who has thus seen the "high authority" of Humboldt doubted, may perhaps learn to doubt of other authorities, and perhaps also learn to fear lest the same measure may not be meted to himself, by future reviewers less humane than ourselves.

Those who please to consider M. Humboldt as very great in any other science, must enjoy their belief, as that is their affair, not ours. But it is our affair, to say that we do not consider him a great geognost, practically, and that we do consider him a very useless writer, not to say more. His book is meant, apparently, as it is declared to be, a system of geology, or geognosy, or a statement of all the facts of the science that have been ascertained; or a compendium, or detail, of "la geognosie positive." Assuredly, it is not that, in any sense. Of the introductory view, from which we have thus extracted, we can say nothing good or commendable. We have not selected passages to condemn, because they were faulty paragraphs, and, by so doing, attempted to misrepresent the whole. There is no plan nor purpose, and no information, in this long preface, which, in the hands of science and knowledge, might have easily contained what it pretends to do; a general view of the state of geognosy, and of its principal facts and relations. It is a sort of talk very much like the living conversation of the man himself; and we rise from the perusal without well knowing what we have been reading.

We have now, perhaps, said more than enough respecting this preface, yet we cannot part from it without recurring to a subject on which we touched before, and to a passage where the author becomes the critic and *laudator* of the Professor of Freiberg, of whose reign we really think it quite time that we should be weary. It is better to quote the passage than to extract from it.

"Werner, in creating geognostic science, has perceived with an admirable sagacity all the relations under which we should view the independence of the primitive, transition, and secondary formations. He has shown what we ought to observe,—what it is important to know; he has prepared, and foreseen in some degree, a part of the discoveries with which, through him, geognosy has been enriched in countries which he could not visit. As formations do not follow the variations of latitude and climate, and phenomena, observed perhaps for the first time in the Himalaya, or the Andes, are found again, and often with an association of circumstances that seem to be entirely accidental, in Germany, Scotland, or the Pyrenees, a very small portion of the globe, a territory of some square leagues in which nature has assembled many formations, may, (like a true *microcosm* of the ancient philosophers), give rise, in the mind of an excellent observer, to very accurate ideas on the fundamental truths of geognosy. In fact, the first views of Werner, even those which that illustrious man had formed before the year 1790, possessed

a justness that is still remarkable. The learned of every country, even those who show no predilection for the school of Freiberg, have preserved them as the basis of geognostic classifications; and yet what was known, however, in 1790, of primitive, transition, and secondary formations, was founded almost entirely on Thuringia, on the metalliferous mountains of Saxony, and those of the Harz, on an extent of country not 75 leagues in length. The memorable labours of Dolomieu, and Saussure's descriptions of the Alps, were consulted, but these could not have influenced in a great degree the labours of Werner."

We suppress the remainder of this long paragraph, as less important. What we have quoted is sufficient to show that the critic is worthy of the subject, and each of the other. It will also tend to justify our own criticisms; for it is perfectly impossible, that any man really acquainted with geognosy, with the facts known, the history of the science, or its present state, could have formed such a judgment, or written such a paragraph.

Werner did not create "la science geognostique." He did little, and of that little, nearly all has proved to be wrong. There is not one of his general laws that has not been found utterly false; and if he ever became possessed of any facts, he proved that he was incapable of reasoning from them. The whole mass, nearly, of false induction and bad reasoning to be found in this science, is to be traced, directly and indirectly, to Werner and to Freiberg. It was, perhaps, a minor crime, that he taught what was untrue or useless, when he was the efficient cause of the ignorance and bad reasoning of a whole army of followers and admirers, and is still the night-mare of the science. If we have begun to shake off his paralyzing influence in England, it is far otherwise in France and Germany; and we, as far as we can, will not allow M. Humboldt to go on blowing the trumpet, because he himself has no other ideas of geognosy than those which he derived at and from Freiberg. We have as little animosity against Werner, or his ghost, as we have against M. Humboldt personally or impersonally; but we will maintain, that to perpetuate the praises of him whose dogmas are the impediment of a rising science, whose assumed infallibility is the stumbling-block of students, is a crime that demands and deserves reprobation, because it is adding to the weight and immobility of the great obstacle to our progress in that science.

It would require a whole essay like this, instead of a paragraph or two, to examine the details of Werner's demerits in

geognosy, and to prove him in category, wrong. We cannot undertake to do it now, and moreover it is a disgusting and dull office. But what right has M. Humboldt to say, that the observations of Dolomieu "could not excercise any great influence over the labours of Werner?" If he meant it in irony, it would be true enough; since that dull and obstinate man shewed, through his whole life, that he was as incapable of making use of any one fact, and as unamenable to reasoning, as he was ignorant of all the necessary knowledge which was indispensable to the office he had undertaken. Wrapt up in his own unintelligible and impossible theories, he was satisfied with reigning a demi-god or a pope among the gaping herd that surrounded him, swallowing all his dogmas and doctrines, and repaying him with adulation and with volumes of his own "crambe recocta." Dolomieu might have taught him, if he had possessed sense enough to listen, that obsidian and pumice were not formed by water, nor volcanoes ignited by coal; in his own day, he might have learnt something of real geognosy from others than Dolomieu, if he had possessed talents of modesty enough to have been a pupil of those whom he professed to teach.

M. Humboldt also asserts, in a part of the paragraph which we have not quoted, that his great teacher exercised a surprising perspicuity in eliciting the truth from the confined narratives of travellers. Nothing so easy; and his pupils have also profited surprisingly by his example and his perspicacity. Nor any thing so easy as to support his system, or any system, by the same perspicacity. It was only to translate the language of any other geognost into his own, to know what the observer saw better than he knew himself, and the work was done. It is a fashion that is just as efficacious now. But we must take our leave of Werner, to return to M. Humboldt himself.

And we return to his 85th page, to his positive geognosy. This properly constitutes the book, and it is just such a collection of naked facts about rocks, as Kirwan wrote, or might have written, in his equally "admirable" work. If this comprises the whole of Positive Geognosy, we are very unfit reviewers of M. Humboldt's book, since we entertain somewhat different views about what is Positive Geognosy. In all the Sciences, there are things, objects, and there are relations, actions, causes, analogies; categories, in short of different kinds, which have generally been held essential parts of what is called a science. Perhaps, however, we have misappre-

hended the meaning of the term "geognosy," but we do presume, that it implies somewhat more than what we find here, and that the adjective term, positive, does not form our author's justification for what he has given us.

It is indifferent where we take an example of this geognosy. We should have taken the first, which comprises the history of granite, but it is too long for extraction, and it is beyond abridgment. If any of our geological readers choose to read it, we will permit them to judge for themselves, and shall not be very uneasy at the prospect of the judgment which they will pronounce between ourselves and the author, as to the general issue. They may select any other part, or read the whole; and if they can extricate any knowledge out of the mass of confusion, why then we shall envy them. Order, or system, there is none. We are dazzled with references to authors, and references to the new world, and hard names; and when we have laboured through ten pages of porphyry or thonschiefer, it is all to be done over again; and when it is done, we ask ourselves what it is all about.

In truth, we did expect some single passage as an example; but, as far as the 240th page at least, we have turned backwards and forwards in vain, and cannot extract one sole paragraph which comprises an entire within itself. The author seems always breathless with matter, and the matter is itself breath, wind. A single fact, well detailed and simply stated, and, if the author ever did reason, reasoned on, would have been worth whole pages of Conradswalde, Prausnitz, Mondragon, Goldlauter, Schwartz, Kiffhauser, Caxamarca, and the ten thousand other names which dazzle our eyes at every sentence, and transport us, in an instant, from Saxony to Chili, as if we had been sitting on Prince Houssain's carpet.

We really cannot see the purpose of all this geography and authority, unless it be to show that M. Humboldt has got a map of the world at his elbow, and is a great traveller. Cannot he see that this is nothing more than an affair of Habitats, and that when once it is ascertained that the general relations of rocks are analogous or similar throughout the globe, it is unnecessary to torment us with every spot of his two hemispheres. The young botanist fancies he has performed a vast feat in science, when he has published in some journal, a list of the plants in the king's park at Edinburgh, or in Norfolk. And it is a very fit occupation for a philosopher in the "Lovely Science." But that granite grows in the Fichtelgebirge, and at Tehuilotepetec, and that it consists, in both

of mica, feldspar, and quartz, or of more mica and less feldspar, is not now very marvellous information; and it is still less necessary that we should be told of every "gisement" in M. Humboldt's hemispheres, and of all and every variety and variation of every thing that he has seen or not seen. Were any general laws deduced, we could understand a purpose in all this; but, as it is, it is egregious trifling, and trifling which pretends to knowledge. Foxes grow in Leicestershire and in Greenland, and so they do in India, and, for ought we care, they may grow upon Popocatepetl; but in what way does the Science of zoology profit by being told that their tails are an inch longer in Mexico than in Bengal, or that they are less odoriferous in Norfolk than at Santa Fé da Bagotá.

The article Porphyry, positively dances before our eyes; and, what with transition, and dolerite, and trachyte, and all else of this never ending subject, we are utterly unable to conjecture what he means to prove, or what he means. Ten sentences might possibly have told his meaning, they would have told ours; but he is not a man of ten sentences, and if he ever meant any thing, he has lost sight of it, suffocated under the rubbish of his talk.

But we have arrived at the 360th page, and still we have been unable to extract one quotation by which we might convey an idea of that talk. Our readers must really oblige us by undertaking a task to which we find ourselves unequal. It is a truth, and to us a sad one, that we really have read the book twice through, so that we cannot be accused of a breach of duty; and it is not less true and sad, that it was less intelligible the second time than the first. Passages, facts, particulars of all kinds, we might criticise beyond endurance, but for what end? If, in our general remarks, our readers should esteem us severe, we will say, read, understand, explain; and we may say too, as Johnson said to Garrick, "If I have told the truth, Davy, why dost thou snub me?"

But there are more last words, and they will require a few last words from us.

We have already alluded to an air of *pretension* which pervades our author's writings; and, among other matters, to his algebra, forming one, but by no means all of the modes in which it is displayed. We mean it for all whom it may suit, and not for him alone, when we say that this use of symbols and equations when the proportion can be given in common language, is either a piece of

miserable affectation, or else, what is vulgarly called priggism, a quality for which we possess no genteeler term. He who makes use of x and y when he might say fifty and a hundred, or square and round, because he happens to have arrived as far as biquadratic equations, wishes to show the world that he knows what algebra is, and would fain be thought a mathematician. The mathematician who, similarly, abuses algebraic language is either a *prig*, or else has been so long versant in the conjuration of *differences*, that he has lost the power of thinking, and the use of his own language. In the last case alone, he is pardonable. The purpose of algebra, of symbols, is abbreviation; it is that we may acquire the power of condensing a long proposition into a small space, so that we may see all its relations at a glance. It is short hand and no more. If the proposition is not of a nature to require this, from its shortness, or other causes, algebraic symbols serve no purpose but to make a plain thing obscure, and to convey an air of mystery. He who thus uses them, instead of proving his knowledge of algebra, betrays his ignorance as well as his affectation; he shows, perhaps, that he knows what substitution is, or what is a simple equation; but he shows, too, that he is a mere mechanic in this art.

Now M. Humboldt, as might be expected, has invented a pasigraphy, "a pasigrafia geognostica," since he must also tell us that he has published in Spanish and in Mexico, or has "elevated himself to general ideas" on geognosy by means of α, β, γ, δ. "This method" also "is double, it is either figurative (graphic, imitative,) representing the superposed beds by parallelograms, placed one above the other; or *algorithmic* indicating the superposition of rocks, and the age of their formation, as the terms of a series." And here, also, he takes occasion to tell us that he has published a book "sur l'irritation de la fibre nerveuse," or "Versuche über die gereizte Muskel und Nervenfaser," by which means we know that he writes in German as well as Spanish and French, and understands the nervous system as well as he does economy, geognosy, and astronomy.

"I published," "I had," "I did," I, the Great, meets us every where; and we believe, indeed, that there is not one of his books which does not contain an enumeration of all the prior ones, under some pretence or other. We have not the least objection that he, or any man, should write on all subjects, provided he understands them; but when we see

this palpable betraying, we draw the natural conclusion that he does not write from the overflowings of his mind, but because he would be thought capable of writing on every thing. As to the purpose of this geognostical pasigraphy, or any purpose to be effected by it, we really cannot explain them, and shall therefore extract one passage, that our readers may form their own judgments.

"If the letters of the alphabet represent these superposed rocks, of two series,

$$\alpha, \beta, \gamma, \delta \ldots .$$
$$\alpha, \alpha\beta, \beta, \beta\gamma, \gamma, \delta \ldots .,$$

the first indicates the succession of simple and independent formations; granite, gneiss, mica-slate, clay-slate or muschelkalk, sandstone of Königstein (quadersandstein), Jura limestone, and green sandstone with lignite (below the chalk). The second indicates the alternation of *simple* with *complex* formations; granite, gneiss-granite, gneiss, gneiss-mica-slate, clay-slate (pp. 86. 88.); or, to give an example taken from the transition formation (p. 129.), limestone with orthoceratites, limestone alternating with slate, transition slate alone, slate and grauwacke, grauwacke alone, transition porphyry . . . . . In the *complex* formations, that is, in those which present the periodical alternation of several beds, we sometimes distinguish three different rocks, which do not pass to each other in the same group;

$$\text{or } \begin{matrix} \alpha, \beta, \alpha\beta\gamma, \gamma \ldots . \\ \alpha\beta\gamma, \alpha\beta\delta, \beta\alpha\varepsilon \ldots . . , \end{matrix}$$

as in primitive formations, there are alternating beds of granite, gneiss, and mica-slate: so in the transition formation, alternating beds of grauwacke, slate, and limestone, or grauwacke, slate, and porphyry, or clay-slate, grauwacke, and greenstone, constitute one formation. In the transition rocks, as we have stated above, clay-slate and grauwacke alone are not the terms of the series. Those terms are all complex; they are groups, and grauwacke belongs at once to several of those groups. It thence results that the term *grauwacke formation* relates only to the predominance of that rock in its association with other rocks.

"Each class affords the example of independent formations which *prelude* as subordinate beds. If $\alpha\beta\gamma$, or $\alpha\beta$, $\beta\gamma$ indicate the complex formations of granite, gneiss, and mica-slate, or of granite and gneiss, clay-slate and porphyry, porphyry and syenite, marl and gypsum, that is, the formation in which beds of two, and even three rocks alternate indefinitely; $\alpha + \beta$, $\beta + \gamma$, will indicate that gneiss forms simply a bed in granite, porphyry in slate, &c. Then

$$\alpha, \alpha + \beta, \beta, \beta + \gamma, \gamma \ldots .$$

expresses the curious phenomenon of formations which *prelude*

or announce themselves as subordinate beds. Those beds sometimes call to mind the terms that precede (*lower rocks*) sometimes the terms that follow (*upper rocks*). Thus we shall have

$$\alpha, \beta, \beta + \alpha, \beta, \beta \times \gamma, \gamma \ldots .$$

The porphyries and granular syenites of the transition formation penetrate into the red sandstone, forming subordinate beds. If the position of the formation of the valley of Fassa is such as has been lately announced (p. 340.), a preceding term (syenite) passes as far as into the alpine limestone or zechstein; it is the case in the series,

$$\alpha, \beta + \alpha, \gamma + \alpha, \delta \ldots .$$

"When we would apply the pasigraphic notation also to the elements of composed rocks, that notation may indicate, how by the progressive augmentation of the elements of the mass, and chiefly by the insulated crystals, beds are formed by a sort of *interior developement*;

$$abc, abc^2, abc^3, \ldots . abc + c.$$

"We have preferred in this particular case (beds of feldspar in granite, beds of quartz in mica-slate or gneiss, beds of hornblende in syenite, beds of pyroxene in transition dolerite) the letters of the Roman alphabet to those of the Greek, in order not to confound the elements of a rock (feldspar, quartz, mica, hornblende, pyroxene) with the rocks that enter into the composition of complex formations.

"We have hitherto shown that in making abstraction altogether of the composition and physical properties of rocks, the *pasigraphic notation* can reduce the most complicated problems of composition to great simplicity. This notation shows how the same subordinate beds (rock-salt in zechstein and red marl, §§ 28, 29.; coal in red sandstone, zechstein, and muschelkalk) pass across several formations, superposed on each other:

$$\alpha + \mu, \beta + \mu, \gamma, \delta + \mu \ldots .$$

It also reminds us of the return of feldspathic and crystalline formations in the transition rocks and red sandstone (Norway, Scotland), a return which is analogous to that of granite after gneiss and primitive mica-slate:

$$\alpha, \beta, \alpha, \gamma, \delta \ldots . . \varkappa, \lambda, \alpha, \beta, \ldots$$

"The first terms of the series re-appear, even after a long interval, after grauwacke and limestone with orthoceratites, that is, after *fragmentary* and *shelly* rocks."

Now we appeal to our readers, if we have judged M. Humboldt harshly, or whether this is not the very grimace of affectation. If he could reason for one moment, or if he had ever considered the real purpose and use of an arithmetical or algebraical pasigraphy, he would see that he was obscuring what he pretends to illustrate, and increasing labour in-

stead of shortening it. A series summed in this manner, conveys no ideas; and if it is to convey any, the labours of the reader will be, to re-translate all that M. Humboldt has exerted so much useless labour in darkening. We only wonder that he has not given us his geognosy in hieroglyphics, that he might have tried to persuade us he was the rival of Young and Champollion. One advantage, indeed, we will not deny; and we are only sorry, therefore, that his whole hemispheres were not written in Algebra. Each method is equally free of the suspicion of conveying ideas, and the Greek would have been shorter than the French.

Geognosy, indeed, can now be contained in a nutshell, and by a shorter process than the Iliad was crammed into one. And lest our readers should doubt the possibility of this, we shall give them the whole matter of the "two hemispheres" in two lines. But unless we gave them the whole passage, they might suspect us of playing with their credulity, and of jesting with M. Humboldt. Here it is;

"In order to unite the principal phenomena of the position of rocks in the primitive, intermediary, secondary, and tertiary formations, I propose the following series:

$\alpha,\ \alpha\beta,\ \beta+\pi,\ \beta\gamma,\ \gamma+\tau,\ \alpha,\ \gamma,\ \delta,\ \alpha,\ \beta,\ \delta,\ o,\ \|\ \varkappa^{g},\ \tau',\ \delta\tau',\ \delta',$
$\delta'+\pi,\ \gamma,\ \tau',\ \sigma\pi,\ \sigma+\alpha,\ \sigma\pi,\ o\ \|\ \pi\varkappa^{a}+\xi,\ \tau^{a}+\vartheta,\ \varkappa^{n},\ \tau^{m},\ \varkappa^{q},$
$\tau^{o},\ \varkappa^{l},\ \tau^{e},\ \|\ \varkappa^{2l},\ \tau^{p}\ldots.$

"It would be useless to give the explanation of those characters; it will be seen by comparing them with the table of formations. I shall confine myself to fixing the attention of the reader on the accumulation of porphyries (π) on the limits of the transition and secondary formations, the position of euphotide formations (o), the great deposites of coal and lignite (ξ), and on the return (almost periodical) of feldspathic formations of transition granite, gneiss, and mica-slate (α, β, γ). As the notation I here present may be variously graduated by the manner in which the characters are accentuated, in uniting them as coefficients in complex formations, or in adding exponents, I doubt whether the names of the rocks arranged by series at the side of each other would address itself as forcibly to the eye as the algorithmic notation."

We must now take leave of the book and the author. We must be sorry that we have been obliged to write such hard truths, and yet we fear that were the cause to occur again, we should do it again. We have said that it was a justice we owed to others, to show that M. Homboldt was not the only authority, or the highest, in every subject of science. And it

is not fitting or just, that the public, which cannot judge, and which necessarily follows the cry of the day, should measure any man by such an imaginary standard, and set up a false God to worship, to the abasement of all others. It is an age of monopolies, but it is hard that the principle of monopoly should be extended from fish to fame, from tea and porter to geognosy and botany, and to all else which ought to be the common property of the republic (which we hope it will ever continue,) of science and letters.

To himself, we might suggest, that, with more accuracy, less *pretension*, better writing, and far less writing he might have acquired a fair share of permanent reputation; but he has desired to be more than all, and will live to see his honors melt from him as snow before the summer. To retrieve is perhaps now impossible; but if he shall shew any desire to do so, we will be among the first to allot the praise, which, he ought now to know, must follow solidity and accuracy, and can never long follow any thing else.

ART. VIII.—*Truité élémentaire de Minéralogie*, par F. S. Beudant. Paris. 1824. pp. 856.——*An Elementary Treastise on Mineralogy*, by F. S. Beudant. (W.)

MANY of the remarks in the preceding article, are applicable to this work; the author is deeply infected with the mania of fabricating new names, and of unnecessarily multiplying genera and species; but as it contains much that is valuable to the mineralogist and artist, we shall proceed to offer some account of it.

M. Beudant is professor of Mineralogy in the academy of Sciences at Paris, and is the author of an elementary treatise on the physical sciences, a voluminous work on Hungary and various important and interesting memoirs which have been published in the scientific journals of Europe. The present work is a part of his course on the physical sciences, and will, we are led to expect, be followed by a treatise on chemistry. It is divided into four distinct parts or books.

The first book comprehends the facts and observations upon which the science of mineralogy is founded, the external characters, the physical properties, and chemical nature of minerals. The second book is devoted to an exposition of the peculiar system of classification and nomenclature adopt-

ed in the work. M. Beudant proposes to divide minerals into three groups or classes, for which he offers the following names; Gazolytes, Leucolytes, and Chroicolytes.

Under the first of these are included those families, the types of which have the common property of existing in an aëriform or gaseous state, or of combining with oxygen, hydrogen, or fluorine, and forming permanent gases,—hence the name Gazolytes, soluble in gas. The second group has also two characters common to the bodies which serve as types to the families, the one being negative and not soluble in gas; tho other being positive and forming *colourless* solutions with acids; hence the name Leucolytes, expressive of this character. The third group includes those bodies which give coloured solutions. The following is a tabular view of this arrangement.

Gazolytes.	Leucolytes.	Chroicolytes.
Silicides,		Tantalides,
Borides,	Antimonides,	Tungstides,
Anthracides,	Stannides,	Titanides,
Hydrogenides,	Zincides,	Molybdides,
Azotides,	Bismuthides,	Chromides,
Sulfurides,	Hydragyrides,	Uranides,
Chlorides,	Argyrides,	Manganides,
Phtorides,	Plumbides,	Siderides,
Selenides,	Aluminides,	Cobaltides,
Tellurides,	Magnesides,	Cuprides,
Phosphorides,		Aurides,
Arsenides,		Platinides,
		Palladides,
		Osmiides.

The Silicides are composed of the oxide of silicium, either alone, or in combination with other oxides, and after fusion with potash afford a compound soluble in acids; the solution yielding on evaporation silica. The Borides are composed of boric acid either alone, or in combination with different oxides. Soluble in alcohol to which they impart the property of burning with a greenish flame.

The family of Anthracides comprises the carbonaceous substances, bitumens, organic salts, as mellite, oxalate of iron, and urate of lime or *guano*. To them succeed the carbonates

of lime, of iron, manganese, zinc, barytes, strontian, lead, silver, bismuth, and copper.

The Hydrogenides, as the name of the family implies, yield water by the action of an alloy of potassium, and thus we have oxide of hydrogen, or water, &c. The nitrates come under the family of Azotides. The sulphurets compose the family of sulfurides, and are subdivided into simple sulphurets, and compound sulphurets, as "sulfo-arseniure de cobalt" or grey cobalt, sulphuret of copper and iron, copper pyrites. To these succeed the sulphates.

We have the same principles of nomenclature followed throughout the work, and the specimens given are sufficient to show that the system is intended to be a chemical one. If the successive discoveries in crystallography, as M. Beudant justly remarks, have freed mineralogy from the empiricism to which it had so long been subjected, it has likewise been brought under more fixed and certain laws, by the researches which have been made into the chemical nature and composition of minerals, within the past six years. These investigations he conceives seem not only to authorize, but to demand an entire new classification of minerals, and with a view of effecting this, the present work appears to have been undertaken. Mineralogists will of course oppose many objections to the proposed classification, but without professing to advocate this new system, we have only felt desirous of giving such a general account of the work as will make it known to our mineralogists. The descriptions of individual minerals which M. Beudant has given, are extremely concise, being for the most part confined to the predominating colour, the crystalline form, and the action of the blow-pipe, and of chemical agents.

As a specimen of our author's manner of describing substances, we may take the following.

"Gazolytes;—family, silicides; genus, silicates; division 1st. aluminous silicates.

Solution freed from silica, forming with solution of caustic potash, a precipitate more or less abundant; sometimes none; the supernatant liquor giving with hydrochlorate of ammonia an abundant gelatinous precipitate (*alumina*) insoluble in carbonate of ammonia, or partially soluble when *glucina* is present.

A. Simple aluminous silicates, a very large solution gives, in general, but a slight precipitate with solution of caustic potash.

SPECIES 1st. *Pinite.*

An opaque substance, tender, compact, or laminated, fusible; crystals derived from a rectangular prism.

Specific gravity 2.93.

Composition $= \dot{A}\,S\,i^2$, 1 atom of alumina 2 of silica; or by weight,

Silica	65
Alumina	35
	100

This substance is often mixed with a greater or less quantity of hydrate of alumina or of hydro-oxide of iron.

Varieties.

Crystallized, cylindrical, laminated, lamellæ disseminated in rocks."

On the subject of crystallization, M. Beudant has noticed several curious facts, and given a new arrangement of crystalline forms, which he conceives are reducible to seven groups or types, viz. 1. The Tetraëdron. 2. The acute or obtuse Rhomboëdron. 3. The straight prism with square bases. 4. The straight rectangular prism. 5. The straight prism with bases of oblique-angled parallelograms. 6. The oblique prism with rectangular bases. 7. The oblique prism with an oblique-angled parallelogram for the base.

In the 6th chapter, M. Beudant proceeds to develope the causes, to the operation of which the great variety of crystalline forms are to be imputed; and which determine any mineral substance to assume one form, or the modifications of that form to the exclusion of others. The solution of these questions is one of the most interesting points in philosophical mineralogy, but it is one of those on which little is known. Our author made numerous researches on natural productions, and many experiments on the salts obtained in the laboratory, and was thus enabled to ascertain three causes having great influence on the crystallization of bodies. These are, 1st. Mechanical mixture of foreign matters in the liquid in which a salt crystallizes, which seems greatly to contribute to the simplicity and regularity of the form; this is also observable in natural crystals, as in those of oxinite, feldspar, &c. when mixed (*remplis*) with particles of mica, and which are more simple than purer crystals in their immediate vicinity.

Secondly, the nature of the liquid in which crystallization

takes place, appears to have much influence on the forms, in various ways, sometimes changing them entirely, at others modifying them by additional facets. Common salt, for example, always crystallizes from its solution in pure water in the cubical form, but in crystallizing from a solution of boracic acid, it always, in the experiments of M. Beudant, assumed the form of a cube truncated on the angles, and consequently passing to the octoëdron. Alum crystallizes from pure water in the form of an octoëdron, but from muriatic acid in the form of the icosaëdron. Very obvious changes are also produced by the addition of a few drops of an acid solution.

As a presumptive proof that anologous principles have operated in nature, it is observed that the same mineral generally affects the same form when accompanied by the same substances, and on the other hand when associated with other substances it presents different forms. The arragonite, for example, which is found in iron mines, is always in very acute pyramids; that on the contrary which is found in the gypseous clay of saliferous deposites, is always in the form of prismatic crystals, grouped in the form of hexagonal prisms.

Thirdly, the substances with which a salt may happen to combine at the moment of crystallizing likewise determine remarkable modifications of the crystalline forms, sometimes reducing the crystals to their most simple forms, and sometimes multiplying the number of their faces. These modifications vary even in the same salt according to the nature of the body with which it is combined. Thus, for example, the sulphate of iron mixed with sulphate of copper, always assumes the form of an oblique rhomboidal prism: sometimes the quantity of sulphate of copper is not sufficient to produce this form, at other times it is sufficient to produce a very near approach to it. M. Beudant has given many illustrations, for which we must refer our readers to the work itself. The discovery of these curious facts promises to form an epoch in the history of Mineralogical Science.

The latter half of this volume is occupied by geological details, so far as they relate to the situation and mode of occurrence of the various minerals described in the preceding part. The second chapter contains a particular description of the substances which enter into the composition of rocks, or which occur in such large masses themselves, as entitle them to be considered as component parts. These are quartz, feldspar, mica, diallage, amphibole, idocrase, garnet, car-

bonate and sulphate of lime. The third chapter contains an account of the non-metallic substances which occur in deposites of small extent, as rock-salt, coal, lignite, and sulphur. To this succeeds a general account of the metalliferous deposites; of those substances which occur in the form of imbedded masses, veins, &c. and of those which are found in the sand and gravel of the newer formations, as the gems, &c.

The fourth book will be found interesting to others than mere mineralogists, as it is devoted to the consideration of the products of the mineral kingdom which are used in the arts, in architecture, jewellery, &c. The curiosity and interest which are excited by the beauties of nature, the investigation of the laws to which its productions are obedient, are unquestionably strong inducements to the study of natural history; but of what importance are all our discoveries, asks M. Beudant, to the mass of mankind?

It is the application of our knowledge of these laws and these productions, to the arts, and to the melioration of human existence, that ought to render them the great object of attention. The mineral kingdom probably furnishes the greatest number of productions which can be applied to useful purposes; offers many resources to the industrious, gives employment to a large proportion of men in civilized life, and yields the most wealth. Architecture draws from it the greatest proportion of its materials, not only what are essential to its structure, but much that is merely ornamental; agriculture derives from it important aid in the improvement of soils; the mechanic arts it supplies with metals, and a vast number and variety of rude materials; nor does it contribute less to the chemical arts, while even luxury daily makes no inconsiderable demands upon it. The number of human being who derive their sole support from the mineral kingdom, is immense; in France alone, which is yet not on a level with its neighbours, the number of men employed in the quarries, mines, and conveyance of mineral substances, is more than three millions. If to these we add all those to whom the mineral kingdom furnishes the raw material, and whose existence depends on this supply, how great will be the number? According to the official reports, these natural products annually furnish an amount of wealth of 933 millions of francs, in rough metallic materials, which the manufacturers and artists increase a hundred fold by their industry, 130 millions of combustibles, more than 500 millions in stones for building, in sand, in

earthy matters for the glass makers and potters, more than 100 millions in different matters for luxury and a multitude of daily wants. Is not all this sufficient to attract our attention to the kingdom which supplies them?

M. Beudant proceeds to take a rapid view of the substances thus employed, commencing with those which are used in building, with a few extracts from which we shall close this notice.

Use of Minerals in the Art of Building.

Though we may use almost every kind of stone in building, it is by no means the case in architecture, properly so called;—it is necessary in architecture, that the materials should be fine enough to admit of sculpture—that they be easily wrought without breaking, with pointed tools and with the chisel,—that they may retain the mouldings, that they be of such tenacity, that the lower parts of the edifice shall not be crushed under the weight of the upper parts; and that at the same time they be not too heavy. It is absolutely necessary also that the vari tions of the atmosphere have no destructive influence over them, that water not only shall not decompose them at last, but even that it shall not penetrate them at all, or rather, that it be not retained within them long enough to be frozen; for by being converted into ice, which occupies a larger extent, it would occasion ruptures in the rock. Finally, it is necessary that these materials be found in extensive beds, that these beds be homogeneous, and free from fissures, that they may be regularly quarried, so that stones of all dimensions may be procured from them.

All these qualities, which architecture imperiously demands, exclude from this species of labor the greatest part of the substances, which form great masses on the surface of the globe, and there are but four kinds, with a considerable number of varieties, that can be used, viz. calcareous rocks, sandstones, volcanic productions, and granitic rocks.

Calcareous Rocks.

The calcareous rocks are most in use, not only because they are most abundant, but because they generally have the advantage of being wrought, and sawed with more ease than any other kind, and at the same time have a sufficient degree of tenacity to resist pressure, to retain angles, mouldings, &c. The varieties are not employed indifferently; some of them have too little cohesiveness, as for example, chalk, in most of its varieties; many granular calcareous rocks, simple or micaceous, of the primitive and intermediate soils, which do not resist pressure. Others, although their parts are perfectly aggregated, are too brittle, too dry, according to the expressive term of the workmen; such are the very compact, fine-grained calcareous stones, with a con-

choidal or scaly fracture; these varieties moreover are frequently full of seams, which diminish their solidity, whether they continue open or whether they have been filled up, and closed with calcareous spar, which has itself but little strength.

The calcareous rocks that have the greatest tenacity and that consequently are best suited to architecture, are generally the compact varieties, whose fracture is uneven, smooth, or irregular, of a dull or earthy lustre; and those that are formed of the remains of shells, confusedly thrown together, and united by a cement which is half crystalline, half earthy. These different varieties are most abundant in secondary and tertiary soils, in deposites analogous to those of the Jura and Paris. These formations have furnished most of the monuments of the civilized world, and the stones taken from them are often transported to a great distance. The finest houses of Amsterdam are built with stones from Schaumberg in Hesse. It is said that the mosques in Constantinople are paved with flag stones, taken from the quarries of Papenheim in Bavaria, on the banks of the Danube, &c. They exist likewise, but in less quantities, in intermediate and primitive soils, which also furnish some crystalline varieties with very small plates crossing one another, of very great tenacity, but which are almost always employed for ornamental purposes.

The rocks of the secondary formations are those usually employed in the south part of Lorrain, in Franche Compté, in Burgundy, in Bourbonnois, on the banks of the Rhone, in one part of Normandy, Poitou, &c. They are also common throughout Germany, and are to be found in some parts of England. The greater number consist of calcareous shells, of a yellowish white, or bluish gray colour, full of encrinites, which belong to the upper part of the Jura; but many are found in the lower parts, of a more compact texture, which at the same time is neither laminated nor too dry; from these may be obtained stones of large dimensions, which present many advantages. The latter were employed in preference to all others by the Romans, in those fine monuments, which still adorn many of the cities in the south of France. A similar stone was used in the construction of the famous Egyptian Pyramids.

* * * *

It is to be observed that most calcareous stones should be so employed in building, as to occupy the same position they had in the quarry from which they were taken; the reason of this is, that the greatest part of the calcareous stones found in beds more or less deep, are made up of small layers which are scarcely perceptible, but which would cause the stone to scale, or to split in a vertical direction, when placed in a different position from its former one in the quarry, or as it is termed, when it is placed in fault (*en delit*). No stones but such as are

of a very compact structure, perfectly homogeneous, forming beds of a great depth, can be placed on either side indifferently.

* * * *

The Granitic Rocks.

The granitic rocks would not be employed in any manner, except in the want of every other kind of stone, or when it is intended to erect monuments as durable as the world; they have solidity that is proof against every thing; witness the numerous Egyptian monuments that have endured for ages, and are quite as fresh as though they were but just finished; but they are very hard, and consequently are hammered with difficulty; they form, at the surface of the earth, very large masses, without stratification, which circumstance indeed furnishes means of obtaining pieces as large in their dimensions, as may be desired; but for the same reason the process of working the quarries is laborious and expensive. Yet in many districts of France no other stone is employed in building; it is this stone that builders are obliged to make use of in a great part of Bretagne, and in Normandy, between Cherbourg and Alençon, in La Marcheana and the adjacent parts of Bourbonnois, Limousin, a part of Auvergne, of Lyonnois (Montbrisson &c.) of Burgundy (Autun &c.), in a great many countries of Europe, and other parts of the world. The quays of the Neva, and the canal of Catherine are of granite; the city and the fortifications of Rio Janeiro are built of granite, as are likewise the towers of the celebrated Chinese wall, &c. This rock is often transported at great expense to places where it is not to be found, either to be employed in union with other materials in the construction of colonnades for large edifices, in which case it is a material for ornamental purposes, or to be used, on account of its great solidity, in masonry that is intended to last forever; for the pavement of streets, quays, all places where the passing is constant, &c.

* * * *

There is but little choice among the granite rocks; all that is necessary is to avoid such as have become altered, those in which the constituent parts exist in large pieces, those in which the mica has a tendency to dispose itself in leaves, and those which are passing into gneiss.

In architecture, granites of different ages, and many kinds of rocks to which geologists give other names, are employed; such as the true porphyries, as in the vicinity of Roanne,* and sienites, as in *les Vosges*, and as seen in many of the Egyptian monuments. Sometimes the name of granite has been given even to the are-

* Our porphyry at Malden is of this kind. W.

naceous rocks of deposites of sea sand-stone, in which all the elements of granite may be found re-aggregated together.

* * * *

Mortar.

Sometimes the different stones, that enter into the construction of a building, are squared with much care on the faces by which they are to join, and are then applied the one upon the other without any intervening substance; but they are generally joined by means of a soft substance, capable of becoming hard after their junction, which is known by the name of cement or mortar. There are many kinds of cement; in the coarse structures of the country, nothing more than clay moistened with water is usually employed, but in more elaborate structures the cement is more carefully selected. It appears that in Egypt, bitumen was often used for that purpose, and we are assured that the walls of Babylon were made of bricks, cemented together by that substance. It has moreover been introduced of late into use in France for small structures, for the common sewers of great cities, for basins, &c. From time immemorial lime has been used, mixed with different substances, reduced to powder, in order to compose the mortar which is to unite the stones in great monuments, and in structures that are intended to possess great solidity. In many places, plaster is used as in the lining of walls, in rough-cast, and in a multitude of cases.

Lime is the result of the burning of the carbonate of lime, an operation which takes the name of calcination, and is extended to many other cases, in which is involved the reduction of a metal, of a sulphuret, or a salt to an oxide. It may be obtained from all the varieties of limestone, but they do not all yield it equally suitable to all kinds of use. In general, lime may be divided in the arts into three kinds, fat, dry, and hydraulic lime. The first of these is inferior to the other two; it absorbs a great quantity of water in slacking, it is long before it hardens even in the open air, and under water, or in moist situations it does not harden at all; yet this kind is that most extensively used, partly perhaps, because it is the most common, and partly because, as it admits of a large quantity of sand, and absorbs a great deal of water, it furnishes much more mortar to a given weight or measure of lime. It is accordingly the most economical, but its cheapness is obtained at the expense of solidity.

We say dry lime is much better suited for building, because it hardens readily in the air, and even in moist places, but it is less economical, for it absorbs but little water, and admits only a small proportion of sand.

Finally, hydraulic lime hardens, not only in moist places, but under water, without the aid of any other substance; it is conse-

quently the last thing that can be employed in all hydraulic structures, in foundations that are situated in wet places, &c. * * * *

It has been thought for a long time that in the drying of a cement, the lime attracts carbonic acid from the atmosphere, and that the mass acquires its greatest solidity when the whole is converted into a carbonate. It is for this reason, it has been said, that the cements which we find in the ancient Roman monuments, possess at this day a greater solidity than our own; but this is an error; the Roman cements do not contain the quantity of carbonic acid necessary to saturate the lime; and hence it is evident, that there exist silicates and aluminates of lime, which are formed at a time posterior to the use of the substance. It is even to be remarked that the hardness of the ancient cements is greater in proportion as the quantity of carbonic acid is less. These observations may serve to show how important it is in choosing, to give the preference to siliceous and aluminous limestones, or when these cannot be procured, to prepare the compounds, that it is necessary to introduce, with great accuracy.

ART. IX.—*A New Theory of Parallel Lines.* By Warren Colburn, Esq.

THE following proposition, on which the theory of parallel lines in its present form depends, has always been considered as not sufficiently evident to be taken as an axiom; but no demonstration has ever been given sufficiently simple to be introduced into an elementary treatise. Consequently, all elementary treatises are defective in this point.

Proposition. If a straight line meets two straight lines so as to make the two interior angles on the same side less than two right angles, these straight lines being continually produced, will at length meet upon that side on which the two angles are less than two right angles.

Upon this, Simson remarks, "the proposition which is usually the 5th postulate or the 11th axiom, by some the 12th, on which this 29th depends, has given a great deal ado to geometers. It seems not properly placed among the axioms, as indeed it is not self-evident, nor does it admit of a demonstration in a strict sense, &c." Simpson's Euclid, Note to Prop. 29th, Book I.

Lacroix, in his geometry, in a note to No. 40, thus remarks. The imperfection of the theory of parallel lines, resides in the difficulty of proving directly this proposition. Many authors

have made unsuccessful attempts to prove it; and others, as Bezout, have endeavoured to conceal the faults of the reasoning; which seems to me contrary to the rigorous duty which every author of elementary works imposes upon himself, &c.

Lacroix gives in a note, a demonstration by M. Bertrand, but it is not sufficiently elementary for learners.

Legendre gives a kind of mechanical illustration of it in the text, and an analytic demonstration sufficiently difficult in a note.

In examining the subject, it appeared to me that the difficulty arose from the definition which did not seem to be the most general or the most natural. I therefore changed the definition for the one given below; which is more general, for the old one follows as a corollary from it, and I believe it will generally be granted to be more natural.

I then endeavoured to prove the usual propositions relating to parallel lines, without using the contested proposition, leaving that for a later period when I should have more data. How well I have succeeded, mathematicians must decide.

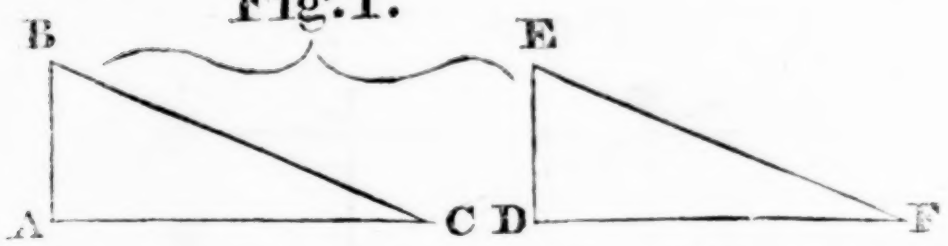
My demonstrations are not longer or more difficult than those usually found in elementary works on the same subject. After my own demonstrations relating to parallels, I have introduced several propositions from Lacroix, in order to obtain data to prove the contested proposition, which is inserted last. The whole so corresponds with Lacroix, that it might be inserted into his work, by omitting such of his demonstrations as relate to the same points, and placing No. 50, 61, and 62 after the proof of the disputed proposition.

A series of propositions might be selected from Legendre, by which the disputed proposition might be proved, using my demonstrations first, but it would destroy the present arrangement of the work.

1. If in any two triangles *ABC* and *DEF*, (fig. 1.) right-angled in *A* and *D*, the sides *BC* and *EF*, opposite the right angles be equal, and a side *AB* in the one, equal to a side *DE* in the other, the triangles are equal.

Demonstration. If *AB* be laid upon *DE* so that *A* may coincide with *D*, the point *B* will coincide with *E*, and the line *AC* will coincide with *DF* on account of the equal angles *A* and *D*. Now *BC*, being equal to *EF*, must meet *DF* at the same distance *DF* from the perpendicular *ED*. Therefore, *AC* is equal to *DF*, and the angles *B* and *C* to *E* and *F* respectively.

Fig. 1.



2. If in any two triangles *ABC* and *DEF* (same figure,) the sides *BC* and *EF*, opposite to the right angles, be equal, and one angle as *C* in the one be equal to the angle *F* of the other, the two triangles are equal.

Demonstration. If *BC* be laid upon *EF* so that *C* may coincide with *F*, the point *B* will coincide with *E*; and the angle *C* being equal to *F*, the line *AC* will coincide with *DF*. Now from the point *E*, *only one perpendicular to the line* DF *can be drawn.* Therefore the line *BA* will coincide with *ED*, and be equal to it. Also *AC* will be equal *DF*, and the angle *B* to the angle *E*.

Definition. *Two straight lines so situated as to be equidistant throughout their whole extent, are called parallel lines.*

Corollary. Parallel lines, if produced to any extent in either direction, will not meet.

It is proposed to prove the following propositions relative to parallel lines.

When two parallel lines *AB* and *CD* (fig. 2.) are cut by a straight line *EF*:

1st. *The alternate internal angles are equal.**

2d. *The alternate external angles are equal;*

3d. *The corresponding angles are equal;*

4th. *The internal angles on the same side are together equal to two right angles.*

5th. *The external angles on the same side are together equal to two right angles.*

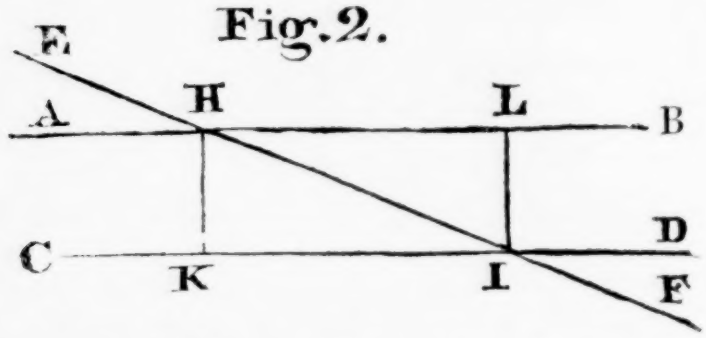
6th. *When two straight lines are so situated as to have either of the above properties, they are parallel.*

Demonstration. 1st. When the line *EF* is oblique. From the points *H* and *I* where the line *EF* cuts *AB* and *CD*, draw *HK* perpendicular to *CD*, and *IL* perpendicular to *AB*.

In the triangles *HIK* and *HIL*, right-angled in *K* and *L*, the line *HI* is common, and opposite to the right angle in each, and the line *HK* is equal to *IL* by the definition; therefore the two triangles are equal in all their parts. Hence

* The names of the angles, adopted here are those of Lacroix. They will be easily understood by referring to the figure.

Fig. 2.



1st. The angle $HIK = IHL$; (these are *alternate internal angles.*)

The angle $HIK + HID$ = two right angles, also

The angle $IHL + IHA$ = two right angles; hence

The angle $HIK + HID = IHL + IHA$;

Taking away the equal angles HIK and IHL. there remains $HID = IHA$; (these are also alternate in-internal angles.)

Therefore the alternate internal angles are equal.

2d. and 3d. The angle $AHE = IHL = HIK = DIF$, and

The angle $EHL = AHI = HID = FIK$; therefore,

The alternate external angles are equal, and the corresponding angles are equal. (The angles EHL and HID and all others situated like them, are *corresponding angles.*)

4th. The angle $AHI + IHL$ = two right angles; but $HIK = IHL$, therefore. $AHI + HIK$ = two right angles; that is, the internal angles on the same side are together equal to two right angles.

5th. The angle $AHE + KIF = HIK + AHI$ = two right angles; that is, the external angles on the same side are together equal to two right angles.

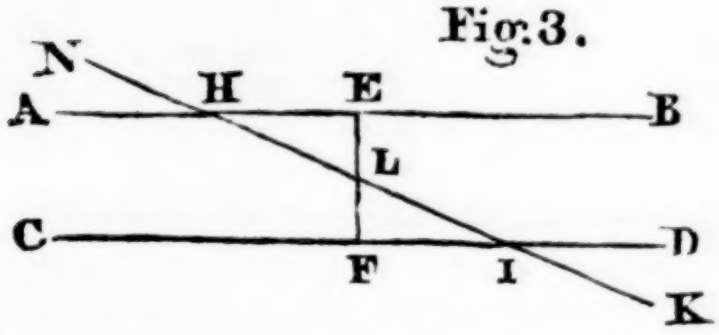
6th. In the same figure, let the straight lines AB and CD be so situated, that, when cut by the straight line EF in any two points H and I, the two interior angles on the same side AHI and HIC may be together equal to two right angles.

From the points H and I, draw HK perpendicular to CD, and IL perpendicular to AB.

The angle $AHI + HIC$ = two right angles by hypothesis, also the angle $AHI + IHL$ = two right angles; therefore, the angle $IHL = HIC$. Hence the triangles HIK and HIL right-angled in K and L, have the angles HIK and IHL equal, and the line HI common and opposite to the right angles; therefore the two triangles are equal in all their parts. Hence the line $HK = LI$. The lines AB and CD, being equidistant in the two parts HK and LI, must be equidistant throughout; that is, they are parallel.

2d. When the line *EF* is perpendicular to one of the lines;

Fig. 3.



Let *AB* and *CD* (fig. 3.) be two parallel straight lines, and cut obliquely by the straight line *NK* in the points *H* and *I*. From *L* the middle of the line *HI* draw *LE* perpendicular to *AB*, and *LF* perpendicular to *CD*. In the triangles *LHE* and *LIF*, right-angled in *E* and *F*, the angle *LHE* = *LIF*, being alternate internal angles, and the lines *LH* and *LI*, opposite to the right angles, are equal by construction. therefore the two triangles are equal, and the angle *HLE* = *ILF*.

The angle *HLE* + *ELI* = two right angles, and because the angle *HLE* = *ILF*, the angle *ILF* + *ELI* = two right angles. Therefore the line *EF* is a straight line and perpendicular to both *AB* and *CD*, from which it will follow that the properties, which were proved for the oblique line, are true for the perpendicular.

The Problem for drawing parallel lines may follow here.

Theorem.

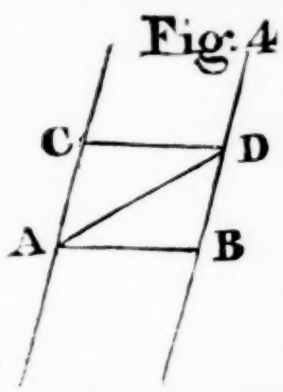
The three angles of a triangle are together equal to two right angles.

For a demonstration, see any treatise on geometry.

Theorem. Lacroix, No. 54.

The parts *AC* and *BD*, (fig. 4.) of two parallel straight lines intercepted between two parallel straight lines are equal to each other, and reciprocally.

Fig. 4



Demonstration. If the line *AD* be drawn, two triangles *ABD* and *ACD* will be formed, which will be equal; for taking *AD* as secant to the parallels *AB* and *CD*, the angles *BAD* and *ADC* are equal, being alternate internal angles; taking *AD* as secant to the parallels *AC* and *BD*, the angles *ADB* and *DAC* are equal for the same reason; farther, the side *AD* being common to the two triangles *ABD* and *ACD*, the triangles are equal. Therefore the sides *AC* and *BD* are equal, which was to be demonstrated. The same is true of the sides *AB* and *CD*.

Reciprocally, if the parts *CD* and *AB* are equal, and also

the parts *AC* and *BD*, the triangles *ABD* and *ACD* will have their three sides equal each to each, and will consequently be equal. The equality of the alternate internal angles *CAD*, *ADB* establishes the parallelism of the straight lines *AC*, *BD*, and the equality of the angles *BAD*, *CDA*, that of the straight lines *AB*, *CD*.

Theorem. *Lacroix*, No. 56.

If any two straight lines *AF* and *GM* (fig. 5,) are so situated as to be cut by a number of parallels *AG*, *BH*, *CI*, &c. in such a manner that the parts of *AF* intercepted between them are equal, the parts *GH*, *HI*, *IK*, &c. of the second will also be equal to each other. The demonstration is varied a little from that of Lacroix.

Demonstration. Through the points *A*, *B*, *C*, &c. draw the right lines *AN*, *BO*, *CP*, &c. parallel to *GM*, the triangles *ABN*, *BCO*, *CDP*, &c. will be formed, in which the sides *AB*, *BC*, *CD*, &c. are equal by hypothesis; the angles *ABN*, *BCO*, *CDP*, &c. are equal, being corresponding angles with regard to the parallels *BH*, *CI*, *DK*, &c., cut by *AF*; the angles *BAN*, *CBO*, *DCP*, &c. are equal, being corresponding angles with regard to the parallels *AN*, *BO*, *CP*, &c. cut by the line *AF*. Consequently the triangles *ABN*, *BCO*, *CDP*, &c. are equal; from which it follows that the lines *AN*, *BO*, *CP*, &c. are equal to each other. But the lines *GH*, *HI*, *IK*, &c. are respectively equal to *AN*, *BO*, *CP*, &c. being parallels intercepted between parallels, therefore *GH*, *HI*, *IK*, &c. are equal to each other.

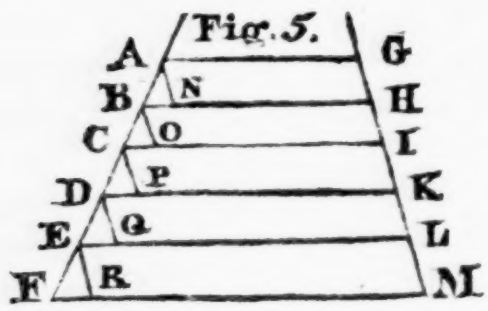


Fig. 5.

Corollary. *Lacroix*, No. 57.

It follows from what precedes, that *AB* is contained in *AF*, as many times as *GH* is contained in *GM*; hence we have this proportion, (or equation.)

$$\frac{AF}{AB} = \frac{GM}{GH},$$

or multiplying by *GH*, and dividing by *AF*,

$$\frac{GH}{AB} = \frac{GM}{AF};$$

Also,

$$\frac{2GH}{2AB} = \frac{GM}{AF};$$

$$\frac{3GH}{3AB} = \frac{GM}{AF}.$$

That is, any number of parts of AF bears the same proportion to the same number of parts of GM, that the whole line AF bears to the whole line GM.

Theorem. *Lacroix*, No. 58.

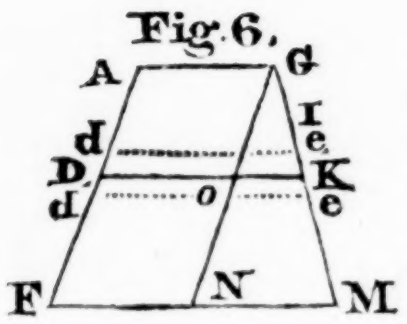
If three parallels AG, DK, FM, (fig. 6,) be cut by any two straight lines AF, GM, the parts intercepted between the parallels will be proportional, so that

$$\frac{AD}{DF} = \frac{GK}{KM}.$$

Demonstration. This proposition admits of two cases. 1st. Let AD be commensurable with AF, that is, let them be such that the ratio of AD to AF may be expressed exactly by numbers. Suppose, for example, that

$$\frac{AF}{AD} = \frac{47}{25};$$

Fig. 6.



If we conceive the right line AF divided into 47 equal parts, AD will contain 25 of those parts, and DF, 22. Drawing then through all the divisions right lines parallel to AG, the right line GM will be divided into 47 equal parts, of which 25 will compose GK, and 22 will compose KM; hence

$$\frac{AD}{DF} = \frac{25}{22},$$

$$\frac{GK}{KM} = \frac{25}{22};$$

from which it follows, that

$$\frac{AD}{DF} = \frac{GK}{KM}.$$

Farther, because of the equations

$$\frac{AF}{AD} = \frac{47}{25},$$

$$\frac{GM}{GK} = \frac{47}{25},$$

we obtain

$$\frac{AF}{AD} = \frac{GM}{GK}.$$

2d. If AF and AD are incommensurable, it may be proved as follows, that their ratio can be neither smaller nor larger than that of GK to GM.

Suppose, first, that

$$\frac{AF}{AD} = \frac{GM}{GI}, \text{ which gives } \frac{AF}{GM} = \frac{AD}{GI},$$

GI being smaller than GK. The side AF may always be divided into parts so small, that lines being drawn through all the points of division parallel to FM, one of them de will pass between the points I and K; we shall have according to what precedes, on account of the commensurability of AF and Ad,

$$\frac{AF}{Ad} = \frac{GM}{Ge},$$

or

$$\frac{AF}{GM} = \frac{Ad}{Ge}.$$

But by hypothesis

$$\frac{AF}{GM} = \frac{AD}{GI},$$

therefore

$$\frac{AD}{GI} = \frac{Ad}{Ge},$$

or

$$\frac{AD}{Ad} = \frac{GI}{Ge},$$

which result is absurd, for AD is greater than Ad, while GI is less than Ge.

By a similar process, it may be shown that we cannot have

$$\frac{AF}{AD} = \frac{GM}{GI'},$$

GI' being greater than GK. Therefore, since GI can be neither greater nor less than GK, it must be equal to it.

From the equation

$$\frac{AF}{AD} = \frac{GM}{GK},$$

we derive

$$\frac{AF - AD}{AD} = \frac{GM - GK}{GK},$$

or

$$\frac{DF}{AD} = \frac{KM}{KG},$$

or inverting terms

$$\frac{AD}{DF} = \frac{GK}{KM}.$$

Note. If the denominators of the two members are subtracted from the numerators, the numerators will each contain the denominators one time less than before.

1st. *Corollary.* *Lacroix*, No. 59.

If through the point G, the line GN be drawn parallel to AF, we have

$$GO = AD, \quad ON = DF,$$

being parallels intercepted between parallels; and by what precedes

$$\frac{GO}{ON} = \frac{GK}{KM},$$

$$\frac{GO}{GN} = \frac{GK}{GM}.$$

If, therefore, there be drawn in a triangle a right line OK, parallel to one of its sides NM, the two other sides GN and GM will be cut into proportional parts by the right line.

2d. *Corollary.* *Lacroix*, No. 60.

Reciprocally, when a right line cuts two sides of a triangle into proportional parts, it is parallel to the third side.

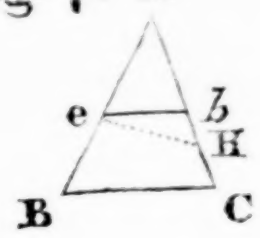
If in the triangle ABC, (fig. 7.) we have

$$\frac{AB}{Ae} = \frac{AC}{Ab},$$

and if eb is not parallel to BC, we can draw through the point e a right line eH parallel to BC, which would give

$$\frac{AB}{Ae} = \frac{AC}{AH}.$$

Fig. 7.



From the two equations, it appears that $AH = Ab$, consequently the lines eb and eH coincide with each other, and that the first is necessarily parallel to BC.

Lacroix, No. 63.

Definition of similar triangles.

Theorem. *Lacroix*, No. 64.

When two triangles *ABC* and *DEF* (fig. 8.) have their angles equal each to each, their homologous sides are proportional, and they are consequently similar.

Demonstration. If we take upon *AB* and *AC*, two parts *A e* and *A b*, respectively equal to *DE* and *DF*, and draw *e b*, the triangles *eAB* and *EDF* will be equal, since by hypothesis the angle *A* of the one is equal to the angle *D* of the other, and the sides *A e* and *A b* are equal to *DE* and *DF* by construction. The angle *A e b* being equal to *E*, will consequently be equal to *B*, and *e b* will be parallel to *BC*, we shall therefere have this equation,

$$\frac{AB}{Ae} = \frac{AC}{Ab} \text{ or } \frac{AB}{DE} = \frac{AC}{DF}.$$

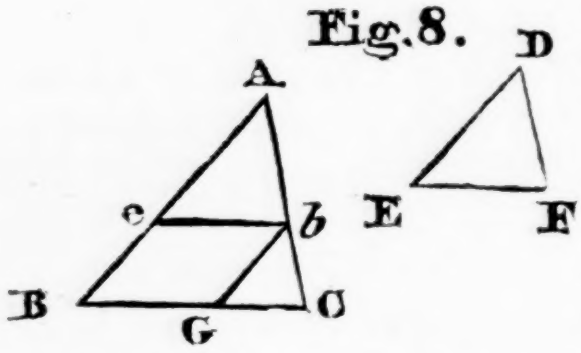
Drawing, then, *GB* parallel to *AB*, we have

$$\frac{AC}{Ab} = \frac{BC}{BG} \text{ or } \frac{AC}{DF} = \frac{BC}{EF},$$

since *BG* is equal to *e b*, which is equal *EF*. Since the member $\frac{AC}{DF}$ is common to both equations, we have

$$\frac{AB}{DE} = \frac{AC}{DF} = \frac{BC}{EF}.$$

Fig. 8.



From which it results, that the homologous sides of the two triangles are proportional.

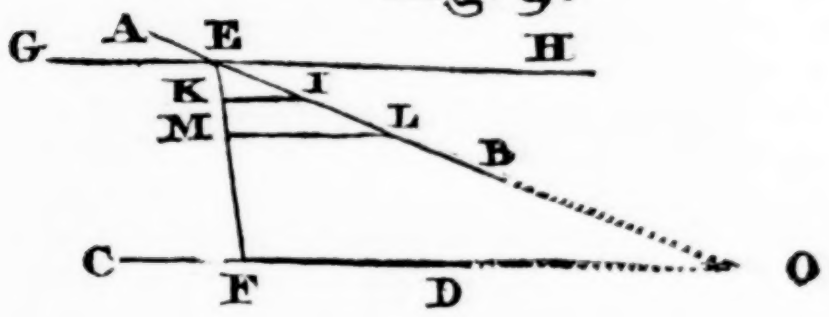
Corollary. *Lacroix*, No. 65.

It follows from the preceding proposition, that two triangles are similar when they have two angles equal each to each, since the third angle in the one is necessarily equal to the third angle of the other.

Theorem.

If two straight lines *AB* and *CD*, (fig. 9.) cut by the straight line *EF* make the angles *BEF* and *DFE* together less than two right angles, they may be produced until they meet.

Fig. 9.



Demonstration. Through E draw GH parallel to CD, also from any point I in the line AB draw IK parallel to CD. The line IK will meet EF in the point K between E and F, for the angles HEF and DFE are together equal to two right angles, being the interior angles on the same side with regard to the parallels GH, CD; and the angles BEF, DFE, are together less than two right angles by hypothesis, hence the angle BEF is less than HEF and the line EB falls within the parallels GH, CD, consequently IK, being parallel to GH, CD, is between them, and a triangle EIK is formed. Through another point L of the line AB, draw the line LM parallel to CD which will meet EF in M, and form the triangle ELM, having its angles respectively equal to those of EIK, consequently these triangles are similar, and we have this equation;

$$\frac{EI}{EL} = \frac{EK}{EM}.$$

If EL is twice EI, EM will be twice EK and so on; that is, the line EB, as it is produced, approaches the line CD produced, exactly in proportion to its length; consequently they may be produced until they meet.

If the ratio of EK to EF be expressed by two numbers, a and b, so that

$$\frac{a}{b} = \frac{EK}{EF},$$

we have

$$\frac{a}{b} = \frac{EI}{EO},$$

which gives

$$EO = \frac{b}{a} \times EI.$$

Whence the point O of their meeting may be found.

General Intelligence.

Pastorff on the Solar Spots and Clouds.—In examining the sun's disk with a fine six-feet achromatic telescope of Fraunhofer, with powers varying from 25 to 400, M. Pastorff of Buchholtz, near Frankfort on the Oder, has observed several interesting phenomena relative to the spots on its surface, their penumbræ, and the phosphoric clouds. He observes that the penumbræ of the spots resemble a mass of the empty eggs of the *Bombix neustria*, which surround the black spots concentrically and with different breadths. These apparent eggs are contiguous, and, as it were, agglomerated the one to the other, with openings extremely small. M. Pastorff considers it quite certain that these spots with the penumbræ are on the surface of the solar globe, and that they disappear when the phosphoric clouds cover them, principally when they are near the margin of the sun; and he thinks that it is probably these phosphoric clouds, which, in the interval of some hours only, form this great variety of spots. On the 1st December, 1823, M. Brioschi of Naples observed a large spot equal to $1\frac{1}{2}$ our globe, surrounded with an irregular and branching elevation, into which there seemed to be precipitating great masses of fire. The whole surface of the sun he saw like an ocean on fire agitated by a storm. M. Pastorff saw this same spot on the same day, when the phosphoric clouds were in great motion; but though he has often seen the agitation of the phosphoric clouds much greater, he did not consider it as resembling an ocean on fire. Almost always, when the spots approach the margin of the sun's disk, they divide themselves into several groups, or they re-unite if they have been previously subdivided. Very near the margin, the spots appear totally altered, and they almost always appear as if they were dissolved and changed into luminous clouds, though that dissolution is only apparent; for it is quite evident that, in proportion as these spots approach the margin of the disk, the penumbra or the nebulosity which encircles them, covers them more and more till they totally disappear. There is then only seen the luminous nebulosity, which is sometimes surrounded with phosphoric

clouds. The sun always appeared more bright at its centre than towards its edge.—*Edin. Jour. of Science.*

Luminous Snow-storm.—Towards the latter end of March, in 1813, a shower of snow fell on Lochawe, in Argyleshire, which alarmed or astonished those by whom it was witnessed, according as they were influenced by superstition or curiosity. The day had been calmly beautiful, but the sky became suddenly gloomy, and in a few minutes a shower of snow fell. The lake (which was of glassy smoothness) immediately presented a luminous surface, forming one huge sheet of fire. A boat, in which were several gentlemen, presented a similar appearance, and the exposed parts of the bodies of the gentlemen seemed to the eye to burn, although without any feeling of warmth. When they applied their hands to any of the melting snow, the luminous substance adhered to them, as well as the moisture, and this property was not lost by the snow for twelve or fifteen minutes.—*Edin. Philos. Jour.*

In the 4th volume of the Memoirs of the American Academy is an account by Prof. Farrar of similar electrical phenomena observed during a snow-storm on the evening of the 17th of January, 1817. This storm extended through a great part of the United States, and was accompanied in some places with almost incessant thunder and lightning. Three persons, crossing the bridge over the Androscoggin river, observed the borders of their hats to be luminous, and when they held up their hands covered with woollen gloves, the ends of their fingers were also luminous. At Williamstown, Vermont, the luminous appearance was seen by a physician on the ears and hair of his horse's head, on the whip of a young man who accompanied him, and on the hat of a gentleman, who, in attempting to brush it off, saw it extend itself over the greater part of his hat. These electrical phenomena were also observed upon the fences and bushes, and upon all substances which extended to any considerable distance above the surface of the ground, and approached to a point, but they had not all the same form. For the most part the light appeared like a spark or star, but in some instances it resembled a blaze, in form of an inverted cone of about two inches in height, and three fourths of an inch in diameter at the base. These blazes emitted a hissing sound, resembling that of the water in a teakettle just before it boils,

which could be heard distinctly at the distance of 6 or 8 feet. They were observed on stakes in the fences, higher than any other substances near them, and when they were removed, the blazes disappeared, and were succeeded by stars.

Appearances similar to the above were said to have been observed in other places on the 10th of March, 1817, and in one instance the light appeared on a well-pole when elevated; upon drawing down the pole, the light gradually diminished, and at length entirely vanished. When the pole was raised again, it reappeared.

In the 1st volume of the Memoirs is an account of a still more luminous effect noticed by Mr L. Baldwin at Woburn, Mass. during a thunder shower in July, 1771. Mr Baldwin having raised an electrical kite, became surrounded with "a fiery atmosphere," which produced a sensation of weakness in his limbs and "a kind of listless feeling." On returning to his house he found his family somewhat alarmed, and was informed that he appeared (during the flying of the kite) to be in the midst of a large, bright flame of fire, attended with flashings; and that they expected, every moment, to see him fall a sacrifice to the flame.

Zircon, a Phosphate of Yttria.—Extract from a letter from M. Berzelius.—I have been tempted, in the course of experiments, which I had undertaken in order to determine the composition of zircon, to ascertain whether thorina was not, as I had for a long time suspected, a hitherto unknown combination of zircon. With this object I employed the last portion remaining in my possession of a specimen, and I have discovered that this pretended earth is nothing more than a *phosphate of yttria* with an excess of the base. By a simple trial with the blow-pipe, I was able to discover the phosphoric acid.

M. Adolph Karl de Weisenfels has discovered that the infusion of one part of white mustard in eight parts of water, is an excellent reagent to discover the presence of alcalis, to the solutions of which it imparts a yellow color.—*Trommsdorff's N. Journal.*

Waerme, Licht, Cometenshweif, Aberration und Messure der Entfernungen der Sterne.——*Heat, Light, and Train of Comets, Aberration, and Measure of Distances of Stars.* By J. Schutz. *Stegermark, Zeitschrift*, p. 90.—The au-

thor examines the hypotheses which explain the light and heat at the surface of the earth, the cold and darkness in the higher regions of the air. The reflection of solar rays at the surface of the earth, in his opinion, contributes but little to the increase of light and heat in low situations; he appears to think that the increase is still less owing to the internal heat of the earth. Here is another hypothesis: every transparent substance, cut into the shape of a lens, refracts the solar rays, so that they are concentrated behind the substance. The atmosphere forms a fluid sphere around the earth, and performs the office of a lens for each point of the earth. The solar rays move in right lines; the farther they may be from the focus, the more they diverge from each other; they become rarefied, feeble, and finally lose their effect. If they fall upon the convex atmosphere of our globe, they are refracted, and concentrated, conformably to the laws of dioptrics, and the farther they penetrate into the atmosphere, the more are they refracted. Thus we may account for the fact, that there is a greater degree of light and heat in low situations, whilst it is always cold and dark in elevated regions. This theory, adds M. Schutz, explains the origin of the trains of comets. These bodies have no train when they are very distant from the sun. The nearer they approach, the larger is the train; moreover this last is always on the side opposite to the sun. Comets are round, aqueous bodies, which receive the light of the sun. The solar rays which fall upon the comet must, by the laws of refraction, become concentrated upon the opposite part of this transparent sphere. This occasions a degree of heat which reduces the water to steam. Above the ether, which may extend itself to a great distance upon our atmosphere, there must be a space devoid of air. Nevertheless, if the water of the comet is converted into vapour in this space devoid of air, by the solar rays, concentrated on one side, this vapour must necessarily move with great rapidity, and appear to us in the form of a train receiving the sun's light. As the concentration of solar rays is always opposite to the sun, the tail of the comet, which always appears on the side opposite to that body, disappears when the comet is so far from the sun that the solar rays are rarefied to such a degree, that they lose their agency.

Application of this theory to aberration. M. Schutz mentions the old explanation and that of Lichtenberg, which he endeavours to refute; then he adds, according to the preced-

ing theory of light, the rays are made to converge by our atmosphere; a star must be seen in the direction these rays have, when they fall upon the eyes. An object seen through a lens may appear out of its actual place; this is the case with the stars, from the effect of the lens of air through which we see them. In the last place, says the author in concluding, this convexity of the atmosphere renders an exact estimation of the distances and magnitudes of the heavenly bodies impossible; for after the rays have been refracted, it is impossible to have the *real* distance or magnitude of any object.

It is unnecessary that we should add, that we merely report the opinions of M. Schutz, without adhering to them at all.—*Ferrussac's Bulletin.*

Preparation of Pure Potash.—Mr Donovan proposes the following as a more easy method of obtaining pure potash than the methods commonly employed.

The crystallized bicarbonate of potash of the shops is to be purified by dissolving it in water at the temperature of 100°. The saturated solution must be filtered and poured into a flat dish, and placed before the fire; in a few hours a crop of crystals of the pure bicarbonate will be obtained. The crystals may then be rinsed with a very small quantity of water, and dried on blotting paper.

The crystals are now to be dissolved in water, and boiled with their own weight of hydrate of lime for 15 minutes; the solution is then to be filtered in the usual manner. We have thus at once a solution of pure potash, without the additional trouble of evaporation and solution in alcohol; and we obviate the reconversion of the alkali into carbonate, which always happens during the evaporation in the common process, unless the tedious and troublesome method of evaporating without contact of the atmosphere be adopted, in which case silver vessels become necessary.

As a test to ascertain whether or not a solution of potash be perfectly caustic, chemists make use of a dilute acid; but this method gives no information unless the acid be added in excess. A small quantity will only displace the carbonic acid from one portion of potash; but the remaining portion will unite with the liberated acid so as to prevent any appearance of effervescence. Thus an alkali that is in fact partly carbonated will not be affected apparently by the affusion of a small quantity of a test acid.—*Dublin Philosophical Journal.*

New Project for a Transporting Apparatus.—Among the numerous projects for improving the methods of transportation, started by the increased interest that the public has lately manifested in rail roads, an invention by Mr Vallance of Brighton, in England, deserves notice, for its strangeness, if not for its ingenuity. It has been called in some journals a mode of "travelling in vacuo." The apparatus consists, essentially, of an air tight tunnel, to be formed from one town to another, about 11 feet in diameter; upon the inside of this a rail way is to be formed, and the carriages adapted to it are to be furnished with a plane, which, like a throttle valve, fills up nearly the whole cross section of the tunnel. It will readily be perceived, that on partially exhausting the air at the end of the tunnel towards which it is intended the carriage shall move; the pressure of the air from the opposite end, acting on the plane fixed to the carriage, will cause it to advance with a velocity corresponding to the degree to which the exhaustion is carried. It is intended to produce the exhaustion by pumps worked, of course, by the steam engine.

Nature of Colour in Mineral Productions.—A correspondent in the Annals of Philosophy was led to doubt of the constant necessity of any specifically colouring ingredient in minerals, from observing that crystallized feldspar exhibits a decided red colour, though analysis points out no substance to which it could be owing; and also from observing that the mineral, after exposure to a strong heat, has the colour entirely destroyed, a colourless glass being obtained. By accident he obtained an artificial mixture of bodies, which exhibited a property of this kind, becoming coloured by particular treatment, though containing nothing to which that colour could be particularly referred. Lime, alumina, silica, soda, and boracic acid, were coarsely mixed together, and exposed to a strong white heat, which produced a semi-vitrified mass of a pure white; a portion of this was finely ground, and after exposure to a low red heat, not above that of melting silver, was found, with surprise, to have assumed a red colour; which colour, with the increase of heat, entirely disappeared, and the substance assumed, at last, as pure a white as it possessed after the first fusion. A lump of the original mass underwent no changes of colour at the same heat, and it was uniformly found, after several trials, that the depth of the colour depended on the fineness of grinding. Nitre destroyed the colour in the fire.

Specimens were sent to the editor, according to whom the pulverised portion, exposed to a low red heat, was of a peach-blossom colour; another portion, exposed to a higher temperature, pale bluish lilac; a third portion, exposed to a moderate white heat, was a slightly greenish white enamel; and a fourth portion, mixed and heated with one fifth nitre, was like the second of the above.—*Ann. Philos.* N.S. IX. 432.

Method of making Transparent Soap.—Tallow is the basis of all soaps for the toilette, known under the name of Windsor, because olive oil forms a paste too difficult to melt, and having an odour too powerful for mixing with perfumes.

Tallow-soap, dissolved with heat in alcohol, returns to its solid state on cooling. It is this fact which has led to the discovery of transparent soap. When well prepared, this soap should have the appearance of fine white sugar-candy. It may also be coloured, and vegetable colours are, for this purpose, preferable to minerals. Any person can make this soap by putting into a thin glass phial half a brick of Windsor soap, cut small, filling the phial half full of alcohol, and placing it near the fire till the soap is dissolved. This mixture, put to cool in a mould, gives the transparent soap.—*Archives des Découvertes et des Inventions Nouvelles.*

On the Employment of the Wood and Bark of the Chestnut-tree in Dyeing and Tanning.—The bark of the chestnut-tree contains twice as much tanning matter as oak-bark, and nearly twice as much colouring matter as logwood. The colouring substance of chestnut-bark is to that of Campeachy logwood exactly as 1.857 to 1.

Leather prepared with this substance is more firm and solid, and yet more supple. This bark is the best substance for making ink; mixed with iron, it becomes a bluish black. The liquor drawn from this bark appears blue at the outside, like indigo; but it gives, on paper, the finest black. In dyeing it has a greater affinity for wool than sumach has, and in other respects it differs but little from sumach and gall-nuts. The colour obtained from this substance is unchangeable by air and light.—*Annales de l'Industrie National.*

Condensation of a Mixture of Hydrogen and Oxygen by pulverulent Platinum.—Dœbereiner has ascertained that *moist* as well as *dry* platinum causes the mutual condensation of these gases. The effect in both cases is equally complete; the only

difference being in the length of time necessary to produce it.

The best method of performing the experiment, is to ignite at the bottom of a glass tube, closed by fusion at one extremity, a quantity of the double ammonio-muriate of platinum, or to decompose in it a solution of platinum by means of a rod of zinc. In either case, a thin film of platinum is deposited upon the interior of the tube, and adheres with considerable firmness. If a tube thus prepared be filled with a mixture of hydrogen and oxygen (or atmospheric air) and inverted over water, the whole of the hydrogen will be condensed into water in the course of a few hours. A similar result is obtained by placing a mass of spongy platinum well soaked with water into a receiver filled with the mixture of the two gases. He next examined what would be the effect of moistening the platinum with other liquids. With alcohol the experiment succeeded equally as well as with water; but not the slightest condensation took place when the spongy metal was imbibed with acid, or with liquid ammonia. He ascribes these differences exclusively to the gaseous mixture being absorbable by water and alcohol, but not by nitric acid or liquid ammonia; in the former case only, the gases would be conveyed into immediate contact with the metal. Dœbereiner concludes with observing, that the existence of some peculiar and independent property in the platinum is more decisively evinced by the present experiment than by any other which he had heretofore made.

These experiments suggested an easy method of depurating hydrogen from minute traces of oxygen. All that is necessary is to enclose it in a stoppered phial, a portion of whose interior has been coated by the process just described, with a thin incrustation of platinum. The oxygen will by degrees undergo condensation.—*Schweigger's Neues Jornal für Chemie und Physik*, xii. 60.

On the Means of testing for Iodine.—Notwithstanding the delicacy of the test for the presence of iodine afforded by the use of starch, its value is considerably diminished by the facility with which various substances interfere with its action; and this is especially the case when any of those bodies, which, either alone or with water, yield hydrogen, are present; the iodine becomes hydriodic acid, and the blue colour either disappears, or is not formed. Sulphurous acid and sulphuretted hydrogen, substances almost constantly pro-

duced by the incineration of organized bodies which contain earthy or alkaline sulphates, are especially capable of producing this effect.

Among the various means of obviating this inconvenience is the use of chlorine, and during his experiments on the products of the Mediterranean, M. Baland has been led to apply it in the following manner:—after having mixed the liquid containing the iodine with the starch and the sulphuric acid, a small quantity of aqueous solution of chlorine is to be added, which from its lightness may be made not to mix with the mixture, but float on the surface; at the place, however, where they touch, a blue zone will be developed where the two solutions are in contact, but if the whole be mixed, it will entirely disappear, if the chlorine be in excess.

By means of this test thus applied, M. Baland has found iodine where it could not before be observed, as in the various marine molluscæ, testaceous or not, such as the *doris*, *venus*, *oysters*, &c. many polypi and marine vegetables, the *gorgonia*, the *zostera marina*, &c. and especially in the mother water of the salt pans fed by the Mediterranean. The small quantity of iodine which is found in sea water, he believes, from the mode of action of the test, to exist as a hydriodate.

M. Baland expresses his conviction that a process of this kind will save chemists the trouble of many fruitless trials, and lead to decided and positive results, where otherwise none would be obtained.—*Ann. de Chimie.*

Anthracite of Pennsylvania.—Mr Maclure. in a letter to Prof. Silliman, remarks, that, this immense formation of anthracite will render this state the most productive in the union; for as soon as they can contrive a good mode of smelting iron ore with the anthracite, this will become one of the greatest iron countries on the globe, because, having so much fine magnetic iron ore, and the natural state of the combustible rendering it capable of producing a very strong heat, without any preparation of coaking or (being) adulterated with any mixture injurious to the making of iron; these circumstances constitute so many advantages as are scarcely to be met with in any one locality as yet known. The benefit that will arise to the nation from a proper method of smelting iron with anthracite, is such an object as to interest the ingenuity of all classes. For the purpose of experiment, it is necessary to procure a furnace on a small scale, as the high furance is too expensive. The fault complained of

being that the mass freezes, or does not retain its fluidity sufficiently, perhaps from the blast being too weak, or too cold or not sufficiently disseminated through the mass; owing, perhaps, to the want of the proper proportions of ore, coal, and flux; the form of the furnace used for charcoal may not be fit for a much greater heat, &c. In short, it may be necessary to try all proportions of blast, fuel, ore, and flux, with all forms and dimensions of furnace.—*Amer. Jour.* No. 21.

Comet of 1825.—We are indebted to R. T. Paine, Esq. for the following account of the late comet.

"When I first saw the comet, it was in the constellation Taurus, and so near the Hyades, that its tail swept over them. In its path it passed from Taurus into Cetus, Eridanus, Cetus again, Fornax Chemica, Apparatus Sculptoris and Phœnix, in which last constellation it was seen for the last time, on Monday evening, Oct. 17th. The following distances it is hoped may be of some small use for the purpose of comparison, or perhaps they may be employed in connection with other observations.

Distance of the comet from (at 10h. 30m. P. M.)

	Oct. 5th.	Oct. 6th.	Oct. 7th.	Oct. 12th.	Oct. 16th.
α piscis aus.	56° 03′	48° 17′	45° 23′	31° 18′	21° 56′
α Androm.	51 32	57 02	57 41		
β. Ceti				14 45	19 41
A. R.	36 15	34 01	31 46	17 45	6 30
N. P. D.	106 47	108 46	111 10	120 30	128 00

Celestial Phenomena for November and December, 1825, for Cambridge, in latitude 42° 23′ 28″ *N. long.* 71° 7′ 25″ *W.* Mean time. (Communicated by R. T. Paine, Esq.)

	D.	H.	M.	S.	
Nov.	2	1	33	15	M. Im. 1st Sat. ♄.
—	3	1	18	08	A. ☌ ☽ 2 a ♋.
—	3	0	8	28	A. ☌ ☽ 1 a ♌.
—	9	2	32	46	M. Em. 3d. Sat. ♄.
—	9	3	26	48	M. Im. 1st. Sat. ♃.
—	12	0	45	30	M. Im. 2d. Sat. ♃.
—	16	2	7	46	M. Em. 4th. Sat. ♃.
—	16	3	2	41	M. Im. 3d. Sat. ♄.
—	16	5	20	16	M. Im. 1st. Sat. ♄.
—	19	3	18	53	M. Im. 2d. Sat. ♄.
—	22	8	18	08	M. ☉ enters ♐.
—	24	6	17	16	M. ☌ ☽ ☌ ♈.

	D.	H.	M.	S.	
Nov.	25	1	42	05	M. Im. 1st. Sat. ♄.
—	25				☽ eclipsed invisible.
—	26	5	52	21	M. Im. 2d. Sat. ♄.
—	26				Occultation of ♄ by ☽.
—	"	7	8	55	A. ☌ in Eliptic.
—	"	5	46	18	A. Im. of ♄ W. Limb, 13′, 2 S of ☽′, C.
—	"	5	41	35	A. Im. of ♄ E. Limb.
—	"	6	9	51	A. Em. of ♄ W. Limb 13′, 5 S of ☽ C.
—	"	6	11	08	A. Em. of ♄ E. Limb.
—	27	11	19	14	A. ☌ ☾ ♈. ♊.

	D.	H.	M.	S.	
Dec.	2	3	35	27	M. Im. 1st. Sat. ♄.
—	8	10	35	40	M. ☌ ☽ ☌ ♍.
—	9	5	23	47	M. Im. 1st. Sat. ♄.
—	9				☉ eclipsed, partly visible.
—	9	3	42	40,1	A. ☌ of ☉ and ☽ in ecliptic.
—	9	3	52	58,2	A eclipse begins.
—	9	4	22	20	A. ☉ sets eclipsed.
—	10	11	57	05	A. Im. Sat. ♄.
—	14	0	16	41	M. Im. 2d. Sat. ♄.
—	18	1	50	23	M. Im. 1st. Sat. ♄.
—	21	2	50	41	M. Im. 2d. Sat. ♄.
—	21	9	01		A. ☉ ent. ♑ winter begins.
—	22	11	57	32	A. ☌ ☽ ☍ ♂, a near approach.
—	22	2	16	13	M. Em. 3d. Sat. ♃.
—	23	4	45	37	A. ☌ ☽ ☌ ♂.
—	23	10	17		A. ☌ ☽ ♄.
—	25	3	43	41	M. Im. 1st. Sat. ♃.
—	25	6	17	36	M. ☌ ☽ ☍ ♊.
—	27	11	37	02	A. ♄ ☽ 1 ☌ ♋.
—	27	11	54	04	A. Im. of Acubens 14′,5 } s. of ☽′
—	28	0	32	49	M. Em. of do. 12,7 } centre.
—	28	5	24	48	M, Im. 2d. Sat. ♃.
—	29	2	48	58	M. Im. 3d. Sat. ♄.
—	29	6	13	30	M. Em. 3d. Sat. ♄.

(*To be continued.*)

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* The moon will first touch the sun 155° 1′ from the highest point of his disc at sunset; 4° 51′ will be eclipsed. At New Orleans 9° 39′ will be eclipsed, at St Augustine 10° 52′ on the south, and at cape Florida 11° 32 on the north limb. The path of the central and total eclipse will strike the United States, near cape Roman on the western, and will leave them near cape East, on the eastern

Additions to the Cabinet of Minerals at Cambridge.

	Locality.	*Presented by*
Pimelite	New Fane	Gen. Field.
Chrysoprase		
Actynolite		
Zoisite		
Chiastolite	Lancaster	Mr F. Plummer.
Sienite, being a portion of "Forefather's Rock"	Plymouth	Mr Spooner.
Chloropheite	Turner's Falls, Gill	Mr Hitchcock.
Fossil oyster	W. Indies	Miss Langdon.
Peruvian Minerals	S. America	Capt. Smith.
Lepidolite	Paris, Maine	J. W. Webster.
Beryls		
Rock crystals		
Red and Green Tourmaline		
Actynolite		
Mica		
Cleavelandite		
Pseudomorphous steatite		
Rose Quartz, &c.		
Scapolite	Bolton and Littleton Mass.	
Spinel		
Pyrallolite		
Petalite, &c.		
Beryls in Quartz	Topsham and Bowdoinham, Me.	
Green Mica		
Tourmaline		
Rose Quartz		
Graphic Granite, &c.		

Botanic Garden, Cambridge.

A list of the rarer and native plants which have flowered in the Botanic Garden at Cambridge during the last two months. [Communicated by Mr Nuttall.]

Aster novæ-angliæ, cyaneus, undulatus, cordifolius, concolor, carolinianus, amygdalinus, puniceus, surculosus, linarifolius, graveolens, multiflorus, sibiricus, chinensis.
Amaryllis Belladonna, falcata, undulata, stellaris.
Acacia pulchella, farnesiana, lophantha.
Baccharis halimifolia.

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side of East Florida. This will be the last eclipse of the sun that will be visible at Cambridge, until the remarkable eclipse of February 12th 1831, which eclipse will be annular at cape Charles in the state of Virginia, at the island of Nantucket in this state, and at Halifax, in the province of Nova Scotia. The next total eclipse of the sun visible in the United states, will happen November 30th 1834, when the sun will be totally eclipsed at the cities of Charleston and Beaufort, in the state of South Carolina.

Campanula pyramidalis.
Casssuarina equisetifolia.
Chelone campanulata.
Cyclamen coum.
Chrysanthemum indicum, (many varieties.)
Dahlia pinnata, (many varieties.)
Diosma ciliaris.
Dracocephalum peregrinum, virginicum, denticulatum, moldavicum.
Erica Plukenetii, triceps, tenella, hirta, spicata, mammosa, transparens.
Epacris juniperina.
Euphorbia neapolitana, meloformis.
Eriogonum longifolium.
Globba marantina.
Hedychium coccineum, maximum, flavum, longifolium, thyrsiforme, fastigiatum, gardenerianum.
Hibiscus syriacus, virginicus, palustris, moscheutos.
Helenium autumnale.
Hioseris radiata.
Ligusticum scoticum.
Melastoma, (from Nepal.)
Mespilus japonica.
Mimulus luteus.
Monarda punctata.
Nepeta nepetella, augustifolia, incana.
Olea fragrans, capensis.
Oxalis versicolor, rubella, rosacea.
Œnothera serruiata.
Pelargonium acetosum, peltatum.
Phylica ericoides.
Phlomis leonurus.
Physalis somnifera, peruviana.
Protea mellifera.
Royena cuneata.
Sesbania macrocarpa.
Silene pendula.
Solanum sanctum, fontanesianum.
Solidago angustifolia, puberula, scabra, altissima, odora, nemoralis, lanceolata, bicolor, cæsia, maritima.
Thea Bohea, viridis.
Veronica australis.
Vitex agnus castus.
Volkameria fragrans.
Yucca gloriosa.
Zimenesia encelioides.
Zinziber chyrysanthum.

ERRATA.

Vol. 2, p. 609, ninth line from top, for 2.39 read 3.2.
" " twentieth line, for *pliable* read *friable*.

ART. X.—*Biographical Account of M. le Comte Legrange.* By M. le Chevalier Delambre. [*Ann. Philos.*]

JOSEPH-LOUIS LAGRANGE, one of the founders of the Academy of Turin, Director during 20 years of the Berlin Academy for the Physico-Mathematical Sciences, Foreign Associate of the Academy of Sciences of Paris, Member of the Imperial Institute and of the Board of Longitude, Senator and Comte of the Empire, Grand Officer of the Legion of Honour, Grand Cross of the Imperial Order of Re-union, was born at Turin, on the 25th of November, 1736. He was the son of Joseph-Louis Lagrange, Treasurer of War, and of Marie-Therese Gros, only daughter of a rich physician of Cambiano.

His great-grandfather, a captain of horse in the service of France, had gone over to that of Emanuel II. King of Sardinia, who established him at Turin by marrying him to a lady of Conti, of an illustrious Roman family. He was originally a Parisian, and a relation of Marie-Louise, dressing maid to the mother of Louis XIV., and afterwards wife of Francis Gaston de Bethune.

These details are of no importance for the illustrious mathematician, whose reputation renders his genealogy of no consequence to him; but they are not indifferent to France, who eagerly recalled him, and restored him to his ancient rights. His name, and that of his mother, show that he was of French extraction. All his works were written in French. The city in which he was born has become a part of the French empire. France therefore has indisputably the right of boasting of one of the greatest geniuses that have done honor to the sciences.

His father was rich, and had made an advantageous marriage; but was ruined by hazardous undertakings. Let us not however lament the situation of M. Lagrange. He himself viewed it as the first cause of all the good fortune that afterwards befell him. "Had I been in possession of a fortune," said he, "I should not probably have studied mathematics." In what other situation would he have found advantages that could enter into comparison with those of a tranquil and studious life, with that splendid series of discoveries in a branch of science considered as the most difficult, and with that personal respectability which was continually increasing to the very last period of his life.

His taste for mathematics did not appear at first. He was

passionately devoted to Cicero and Virgil, before he could read Archimedes and Newton. He then became an enthusiastic admirer of the geometry of the ancients, which he preferred to the modern analysis. A memoir which the celebrated Halley had composed long before, to demonstrate the superiority of the analytic method, had the glory of converting him, and teaching him his true destiny. He devoted himself to this new study with the same success that he had had in the synthesis, and which was so decided that at the age of 16 he was Professor of Mathematics in the Royal Military School. The extreme youth of a Professor is a great advantage to him when he has shown extraordinary abilities and when his pupils are no longer children. All the pupils of M. Lagrange were older than himself, and were not the less attentive to his lectures on that account. He distinguished some of them whom he made his friends.

From this association sprung the Academy of Turin, which in 1759 published a first volume, entitled Acts of a private Society. We see there the young Lagrange directing the philosophical researches of the physician Cigna, and the labours of the Chevalier de Saluces. He furnished to Foncenex the analytical part of his memoirs, leaving to him the task of developing the reasoning upon which the formulas depended. In these memoirs, which do not bear his name, we observe that purely analytical method, which afterwards characterized his great productions. He had found a new theory of the lever. It constitutes the third part of a memoir, which was very successful. Foncenex, in recompense, was placed at the head of the Marine, which the king of Sardinia formed at that time. The two first parts have the same style and seem written by the same person. Do they likewise belong to Lagrange? He never expressly laid claim to them; but what may give us some light into the real author, is that Foncenex soon ceased to enrich the volumes of the new Academy, and that Montucla, ignorant of what Lagrange revealed to us during the latter part of his life, is astonished that Foncenex interrupted those researches which might have given him a great reputation.

M. Lagrange, while he abandoned to his friend insulated theorems, published at the same time, under his own name, theories which he promised to develope further. Thus after having given new formulas of the *maximum* and *minimum* in all cases, after having shown the insufficiency of the known methods, he announces that he will treat this subject, which

he considered as important, in a work which he was preparing, in which would be deduced from the same principles all the mechanical properties of bodies, whether solid or fluid. Thus at the age of 23 he had laid the foundation of the great works, which constitute the admiration of philosophers.

In the same volume he reduces under the differential calculus the theory of recurrent series and the doctrine of chances; which before that time had only been treated by indirect methods. He established them upon more natural and general principles.

Newton had undertaken to submit the motions of fluids to calculation. He had made researches on the propagation of sound; but his principles were insufficient and even faulty, and his suppositions inconsistent with each other. Lagrange demonstrates this. He founds his new researches on the known laws of dynamics, and by considering only in the air the particles which are in a straight line, he reduces the problem to that of vibrating cords, respecting which the greatest mathematicians differed in opinion. He shows that their calculations are insufficient to decide the question. He undertakes a general solution by an analysis equally new and interesting, which enables him to resolve at once an indefinite number of equations, and which embraces even discontinued functions. He establishes on more solid grounds the theory of the mixture of simple and regular vibrations of Daniel Bernoulli. He shows the limits within which this theory is exact, and beyond which it becomes faulty. Then he comes to the construction given by Euler, a construction true in itself, although its first author had arrived at it by calculations which were not quite rigorous. He answers the objections of D'Alembert. He demonstrates that whatever figure is given to the cord, the duration of the oscillations is always the same; a truth derived from experiment which D'Alembert considered as very difficult if not impossible to demonstrate. He passes to the propagation of sound, treats of simple and compound echos, of the mixture of sounds, of the possibility of their spreading in the same space without interfering with each other. He demonstrates rigorously the generation of harmonious sounds. Finally, he announces that his intention is to destroy the prejudices of those who still doubt whether the mathematics can ever throw a real light upon physics.

We have given this long account of that memoir, because it is the first by which M. Lagrange became known. If the

analytical reasoning in it be of the most transcendent kind, the object at least has something sensible. He recalls names and facts which are well known to most people. What is surprising is, that such a first essay should be the production of a young man, who took possession of a subject treated by Newton, Taylor, Bernoulli, D'Alembert, and Euler. He appears all at once in the midst of these great mathematicians as their equal, as a judge, who in order to put an end to a difficult dispute, points out how far each of them is in the right, and how far they have deceived themselves; determines the dispute between them, corrects their errors, and gives them the true solution, which they had perceived without knowing it to be so.

Euler saw the merit of the new method, and took it for the object of his profoundest meditations. D'Alembert did not yield the point in dispute. In his private letters, as well as in his printed memoirs, he proposed numerous objections, to which Lagrange afterwards answered. But these objections may give rise to this question: How comes it that, in a science in which every one admits the merit of exactness, geniuses of the first order take different sides, and continue to dispute for a long time? The reason is, that in problems of this kind, the solutions of which cannot be subjected to the proof of experiment, besides the part of the calculation which is subjected to rigorous laws, and respecting which it is not possible to entertain two opinions, there is always a metaphysical part which leaves doubt and obscurity. It is because in the calculations themselves mathematicians are often content with pointing out the way in which the demonstration may be made; they suppress the developements which are not always so superfluous as they think. The care of filling up these blanks would require a labour which the author alone would have the courage to accomplish. Even he himself, drawn on by his subject and by the habits which he has acquired, allows himself to leap over the intermediate ideas. He divines his definite equation, instead of arriving at it step by step with an attention that would prevent every mistake. Hence it happens that more timid calculators sometimes point out mistakes in the calculations of an Euler, a D'Alembert, a Lagrange. Hence it happens that men of very great genius do not at first agree, from not having studied each other with sufficient attention to understand each other's meaning.

The first answer of Euler was to make Lagrange an asso-

ciate of the Berlin Academy. When he announced to him this nomination on the 20th of October, 1759, he said, "Your solution of the problem of isoperimetres leaves nothing to desire; and I am happy that this subject, with which I was almost alone occupied since the first attempts, has been carried by you to the highest degree of perfection. The importance of the matter has induced me to draw up, with your assistance, an analytical solution of it. But I shall not publish it, till you yourself have published the sequel of your researches, that I may not deprive you of any part of the glory which is your due."

If these delicate proceedings and the testimonies of the highest esteem were very flattering to a young man of 24 years of age, they do no less honour to the great man, who at that time swayed the sceptre of mathematics, and who thus accurately estimated the merit of a work that announced to him a successor.

But these praises are to be found in a letter. It may be supposed that the great and good Euler has indulged in some of those exaggerations which the epistolary style permits. Let us see then how he has expressed himself in the dissertation which his letter announced. It begins as follows:

"After having fatigued myself for a long time and to no purpose in endeavouring to find this integral, what was my astonishment when I learnt that in the Turin Memoirs the problem was resolved with as much facility as felicity. This fine discovery produced in me so much the more admiration, as it is very different from the methods which I have given, and far surpasses them all in simplicity."

It is thus that Euler begins the memoir in which he explains with his usual clearness the foundation of the method of his young rival, and the theory of the new calculus, which he called the *calculus of variations*.

To make the motives of this admiration which Euler bestowed with so much frankness better understood, it will not be useless to go back to the origin of the researches of Lagrange, such as he stated them himself two days before his death.

The first attempts to determine the *maximum* and *minimum* in all indefinite integral formulas, were made upon the occasion of the curve of swiftest descent, and the isoperimetres of Bernoulli. Euler had brought them to a general method, in an original work, in which the profoundest knowledge of the calculus is conspicuous. But however ingenious his method

was, it had not all the simplicity which one would wish to see in a work of pure analysis. The author admitted this himself. He allowed the necessity of a demonstration independent of geometry. He appeared to doubt the resources of analysis, and terminated his work by saying, "If my principle be not sufficiently demonstrated, yet as it is conformable to truth, I have no doubt that by means of a rigid metaphysical explanation it may be put in the clearest light, and I leave that task to the metaphysicians."

This appeal, to which the metaphysicians paid no attention, was listened to by Lagrange, and excited his emulation. In a short time the young man found the solution of which Euler had despaired. *He found it by analysis.* And in giving an account of the way in which he had been led to that discovery, he said expressly, and as it were in answer to Euler's doubt, that he regarded it not as a metaphysical principle, but as a necessary result of the laws of mechanics, as a simple corollary from a more general law, which he afterwards made the foundation of his Mechanique Analytique. (See that work, page 189 of the first edition.)

This noble emulation which excited him to triumph over difficulties considered as insurmountable, and to rectify or complete theories remaining imperfect, appears to have always directed M. Lagrange in the choice of his subjects.

D'Alembert had considered it as impossible to subject to calculations the motions of a fluid inclosed in a vessel, unless this vessel had a certain figure. Lagrange demonstrates the contrary; except in the case when the fluid divides itself into different masses. But even then we may determine the places where the fluid divides itself into different portions, and determine the motion of each as if it were alone.

D'Alembert had thought that in a fluid mass, such as the earth may have been at its origin, it was necessary for the different beds to be on a level. Lagrange shows that the equations of D'Alembert are themselves equations of beds on a level.

In combating D'Alembert with all the delicacy due to a mathematician of his rank, he often employs very beautiful theorems, for which he was indebted to his adversary. D'Alembert on his side added to the researches of Lagrange. "Your problem appeared to me so beautiful," says he in a letter to Lagrange, "that I have sought for another solution of it. I have found a simpler method of arriving at your

elegant formula." These examples, which it would be easy to multiply, prove with what politeness these celebrated rivals corresponded, who, opposing each other without intermission, whether conquerors or conquered, constantly found in their discussion reasons for esteeming each other more, and furnished to their antagonist occasions which might lead them to new triumphs.

The Academy of Sciences of Paris had proposed, as the subject of a prize, the theory of the libration of the moon. That is to say, they demanded the cause why the moon, in revolving round the earth, always turns the same face to it, some variations excepted, observed by astronomers, and of which Cassini had first explained the mechanism. The point was to calculate all the phenomena, and to deduce them from the principle of universal gravitation. Such a subject was an appeal to the genius of Lagrange, an opportunity furnished to apply his analytical principles and discoveries. The attempt of D'Alembert was not disappointed. The memoir of Lagrange is one of his finest pieces. We see in it the first developement of his ideas, and the germ of his Mechanique Analytique. D'Alembert wrote to him: "I have read with as much pleasure as advantage your excellent paper on the Libration, so worthy of the prize which it obtained."

This success encouraged the Academy to propose, as a prize, the theory of the satellites of Jupiter. Euler, Clairaut, and D'Alembert had employed themselves about the problem of three bodies on occasion of the movements of the moon. Bailly then applied the theory of Clairaut to the problem of the satellites, and it had led him to very interesting results. But this theory was insufficient. The earth has only one moon while Jupiter has four, which ought continually to act upon each other and alter their positions in their revolutions. The problem was that of six bodies. Lagrange attacked the difficulty and overcame it, demonstrated the cause of the inequalities observed by astronomers, and pointed out some others too feeble to be ascertained by observations. The shortness of the time allowed, and the immensity of the calculations, both analytical and numerical, did not permit him to exhaust the subject entirely in a first memoir. He was sensible of this himself, and promised further results, which his other labours always prevented him from giving. Twenty-four years after M. Laplace took up that difficult theory, and made important discoveries in it, which completed it and

put it in the power of astronomers to banish empiricism from their tables.

About the same time a problem of quite a different kind drew the attention of M. Lagrange. Fermat, one of the greatest mathematicians of his time, had left very remarkable theorems respecting the properties of numbers, which he probably discovered by induction. He had promised the demonstrations of them; but at his death no trace of them could be found. Whether he had suppressed them as insufficient, or from some other cause, cannot now be ascertained. These theorems perhaps may appear more curious than useful. But it is well known that difficulty constitutes a strong attraction for men, especially mathematicians. Without such a motive would they have attached so much importance to the problems of the brachytochrone, of the isoperimetres, and of the orthogonal trajectories? Certainly not. They wished to create the science of calculation, and to perfect methods which could not fail some day of finding useful applications. With this view they attached themselves to the first question which required new resources. The system of the world discovered by Newton was a most fortunate event for them. Never could the transcendental calculus find a subject more worthy or more rich. Whatever progress is made in it, the first discoverer will always retain his rank. Accordingly, M. Lagrange, who cites him often as the greatest genius that ever existed, adds also, "and the most fortunate. We do not find every day a system of the world to establish." It has required 100 years of labours and discoveries to raise the edifice of which Newton laid the foundation. But every thing is ascribed to him, and we suppose him to have traversed the whole country upon which he merely entered.

Many mathematicians doubtless employed themselves on the theorems of Fermat; but none have been successful. Euler alone had penetrated into that difficult road in which M. Legendre and M. Gauss afterwards signalized themselves. M. Lagrange in demonstrating or rectifying some opinions of Euler, resolved a problem which appears to be the key of all the others; and from which he deduced a useful result; namely, the complete resolution of equations of the second degree, with two indeterminates which must be whole numbers.

This memoir, printed like the preceding, among those of the Turin Academy, is notwithstanding dated Berlin, the 20th September, 1768. This date points out to us one of the few

events which render the life of Lagrange not entirely a detail of his writings.

His stay at Turin was not agreeable to him. He saw no person there who cultivated the mathematics with success. He was impatient to see the philosophers of Paris with whom he corresponded. M. de Caraccioli, with whom he lived in the greatest intimacy, was appointed ambassador to London, and was to pass through Paris on his way, where he intended to spend some time. He proposed this journey to M. Lagrange, who consented to it with joy, and who was received as he had a right to expect by D'Alembert, Clairaut, Condorcet, Fontaine, Nollet, Marie, and the other philosophers. Falling dangerously ill after a dinner in the Italian style given him by Nollet, he was not able to accompany his friend to London, who had received sudden orders to repair to his post, and who was obliged to leave him in a furnished lodging under the care of a confident charged to provide every thing.

This incident changed his projects. He thought only of returning to Turin. He devoted himself to the mathematics with new ardour, when he understood that the Academy of Berlin was threatened with the loss of Euler, who thought of returning to Petersburgh. D'Alembert speaks of this project of Euler, in a letter to Voltaire, dated the 3d of March, 1766: "I shall be sorry for it," says he, "he is a man by no means amusing, but a very great mathematician." It was of little consequence to D'Alembert whether this man *by no means amusing*, went 7 degrees nearer the pole. He could read the works of the great mathematician as well in the Petersburgh Memoirs as in those of Berlin. What troubled D'Alembert was the fear of seeing himself called upon to fill his place, and the difficulty of giving an answer to offers which he was determined not to accept. Frederick in fact offered him again the place of President of his Academy, which he had kept in reserve for him ever since the death of Maupertuis. D'Alembert suggested the idea of putting Lagrange in the place of Euler, and, if we believe the author of the Secret History of the Court of Berlin, (vol. ii. p. 414) Euler had already pointed out Lagrange as the only man capable of filling his place. In fact it was natural that Euler, who wished to obtain permission to leave Berlin, and D'Alembert who wanted a pretext not to go there, should both of them without any communication, have cast their

eyes on the man who was best fitted to maintain the eclat which the labours of Euler had thrown round the Berlin Academy.

Lagrange was pitched upon. He received a pension of 1500 Prussian crowns, about 250*l*., with the title of Director of the Academy for the Physico-mathematical Sciences. We may be surprised that Euler and Lagrange, put successively in the place of Maupertius, received only the half of his salary, which the king offered entire to D'Alembert. The reason is, that this prince, who at his leisure hours cultivated poetry and the arts, had no idea of the sciences, though he considered himself obliged to protect them as a king. He had very little respect for the mathematics, against which he wrote three pages in verse, and sent them to D'Alembert himself, who deferred writing an answer till the termination of the siege of Schweidnitz; because he thought it would be too much to have both Austria and the mathematics on his hands at once. Notwithstanding the prodigious reputation of Euler, we see, from the king's correspondence with Voltaire, that he gave him no other appellation than his *narrow-minded geometer, whose ears were not capable of feeling the delicacy of poetry.* To which Voltaire replies: *We are a small number of adepts who know one another; the rest are profane.* We see well that Voltaire who had written so well in praise of Newton, endeavours in this place to flatter Frederick. He enters out of complaisance into the ideas of this prince, who wished to put at the head of his academy, a man who had at least some pretensions to literature. Fearing that a mathematician would not take sufficient interest in the direction of literary labours; and that a man of literature would have been still worse placed at the head of a society composed in part of philosophers of whose language he was ignorant; on that account he divided the situation, and put two persons in it, that it might be completely filled.

[*To be continued.*]

Art. XI.—*On the Mode of obtaining Silicium, and on the Characters and Properties of that Substance.* By M. Berzelius.

(Concluded from page 45.)

Preparation of Silicium.

The combustion of potassium in silicated fluoric gas requires apparatus which we have not always at hand. The double salts, on the contrary, which fluoric acid forms with silica and potash, or soda, afford very advantageous means of preparing silicium. I did not observe any difference in the use of one or the other of these salts. That of soda has however the advantage of containing under the same weight and the same volume a greater quantity of the fluate of silica. This salt is used in the following manner:—It is reduced to a fine powder; and in case the desiccation has made its particles adhere, and also to expel the humidity that may remain, it is exposed to a temperature as high as it can bear without being decomposed, that is to say, much above 100°. It is then put by layers with the potassium into a tube of glass closed at one end, which is placed in such a manner as that all the mass may be heated at the same time. We may, if we choose, melt the potassium and mix it, by means of a rod of iron, with the salt, and then heat the mass over a spirit-lamp. Even before a red heat, the silicium is reduced with a slight hissing and a feeble appearance of heat. No gas is disengaged when the salt has been well dried. The mass is left to cool, and then treated as before described. It ought however to be dissolved in a great quantity of water; so that the liquid, which becomes alkaline by the oxidation of the potassium, may be as diluted as possible, and have less tendency to oxidate and dissolve the silicium. It is for this reason that the mass ought not to be treated with hot water,—that the liquid has not lost its alkaline quality by the washing. It is afterwards boiled with water, and washed with hot water until no stain is left by the evaporation of a drop of liquid. For this purpose much time is generally requisite, and much water must be used. The silicium obtained by this process contains hydrogen; but in a smaller quantity, however, and perhaps in the same manner as the common charcoal of wood, which Davy considers as hydrogenated carbon. It contains also silica, which proceeds principally from the potassium before the reduction, being oxidated a little, and then sepa-

rating a quantity of silica corresponding to the alkali formed; but the alkali which is formed after the reduction, when it dissolves the mass in the water, dissolves a portion of the double salt in excess, without separating the silica from it. This silica ought to be taken away by means of fluoric acid; but as the silicium would dissolve in the acid, we must begin by rendering it insoluble and incombustible; if it were made to burn in air, a portion of incombustible silicium would indeed be obtained after treatment by fluoric acid; but ordinarily two thirds of it would be lost by the combustion. This loss is prevented by heating nearly to ignition the dried silicium containing hydrogen in an open crucible; this degree of heat is maintained some time, and then it is raised little by little to a perfect red heat. If the silicium become inflamed, the crucible should be covered directly, and the temperature lowered, which will instantly stop the combustion. When the calcination is finished, the silicium is incombustible in air, and is no longer attacked by fluoric acid, if it does not contain any foreign metal, iron, or manganese for example; for in this case the alloy would be entirely dissolved with disengagement of hydrogen. After treatment by the acid, the silicium is washed and dried. It might be thought that this incombustibility is caused by an extremely thin pellicle of silica; but I dried the silicium in a vacuum, and then heated it to redness in the air, and I did not find any change of weight.

The silica may be reduced by heating it with potassium: but it happens either that the combination, rich in potassium, becomes entirely dissolved in the water; or else, when the heat is sufficient to expel the excess of potassium, that the silicate of potash formed melts into a vitreous mass, and envelops the silicium, which thence acquires a clearer colour. Part of the silicate may be separated by means of water, but the remainder can only be removed by fluoric acid. The quantity of silicium is extremely small; and this manner of obtaining it merits attention only because it gives the same result as the double salt when treated with potassium; which proves that the evolution of hydrogen that takes place when the mass is treated by water is owing to the potassium, and not to the combustible radical of fluoric acid. It is by the process in question that Davy endeavoured to reduce silica; and he obtained with the mixed silicate of potash only a brown pulverulent matter, which dissolved in the water, giving it a greenish-gray colour. I observed the same colour in the solution; but it disappears when this becomes clear.

I passed silicated fluoric gas through a red-hot iron tube filled with turnings of the same metal, and it was not sensibly absorbed. The turnings when taken out were of a nut-brown colour, similar to that of silicium, and had the taste of fluate of iron, in the places where the heat had had the most intensity. This last having been dissolved by water, a perceptible pellicle of silicium was left on the surface of the iron, but so thin that it was not possible to separate it. We see by this experiment that iron at a sufficiently elevated temperature has an affinity strong enough to decompose the gas; but that the decomposition soon ceases, because the silicium which is deposited at the surface of the metal prevents its action. By fusing in a covered iron vessel a mixture of very fine iron filings with fluate of silica and potash, the salt was decomposed and converted into double proto-fluate of iron and potash; after having dissolved it in warm water, a combination of iron and of silicium was left. I hoped to be able to separate the iron by means of an acid; but the silicium became oxidated at the same time, although I employed the liquid silicated fluoric acid. In drying this alloy it oxidated more quickly in the air than it dried, and was changed into an ochre of a rusty yellow colour.

Composition of Silica.

Since silicium can be obtained in a direct manner, it was natural to endeavor to ascertain its oxidation in a direct manner. For this purpose I burnt 100 parts of pure silicium, dried in a vacuum, by heating it with carbonate of soda. The mass, treated by muriatic acid, evaporated to dryness, and strongly heated, was dissolved in water, and left some silica coloured gray by carbon, which, well washed and ignited, became snow-white, and weighed 203,75. The solution obtained and the waters of the washing were evaporated afresh, and the saline residuum heated to redness. Treating this with water, it still gave a little silica, which, after the addition of a drop of ammonia, took at the expiration of some hours a colour inclining to a yellowish-brown; when heated to redness it weighed 1,5; it had lost its dark colour, but it was not snow-white. With soda it gave on platinum foil a faint but evident trace of manganese. 100 parts of silicium had consequently absorbed 105,25 of oxygen, and produced 205,25 of silica. The experiment was repeated on a portion of silicium on which fluoric acid had been evaporated, in order to be more certain that all the silica had been remov-

ed. 100 parts of silicium, previously calcined to redness in the air, gave, by the process described above, 207 of silica; and by evaporation of the waters, 1 part; or in all, 208 of silica.

According to these two experiments, silica is composed of

Silicium . . .	48,72 to 48,08
Oxygen . . .	51,28 to 51,92

They both give a greater proportion of oxygen than that which I found, according to the capacity of saturation of silica for saline bases, and which is only 50,3.

But if we return to the analysis of the salts containing fluate of silica, we shall be able to calculate the saturating capacity of silicium according to the results which we have obtained from it.

Of all these double salts, that of barytes is the most suitable. The only uncertainty which could be met with in its analysis arises from its retaining a little humidity which is not disengaged until the moment of its decomposition. But the quantity of it may be determined by melting the salt with oxide of lead; for the acid is retained, and the water only disengaged. 100 parts of the salt lost in this manner 0,85 of moisture. 100 parts of the same salt, weighed at the same time, gave, according to the process described for the analysis of the fluate of silica and barytes, 82,933 of sulphate of barytes, representing 54,428 of barytes. But according to the analysis already known of the double fluate of silica and barytes, the base is combined with three times more fluoric acid than in the neutral fluate. It thence follows that the double salt is formed of

Barytes	54.428
Fluoric acid	22,836
Silica	21,886
Moisture	0,850
	100,000

The 54,428 parts of barytes are saturated by 7,612 of fluoric acid; whence it follows that 15,224 of this acid were combined with 21,886 of silica, or that 100 of acid took 143,76 of silica. The fluate of silica is consequently composed of

Fluoric acid . . .	410,24	100,00
Silica	589,76	143,76

But 100 parts of fluoric acid corresponds to 74,7194 of oxygen in each base that it saturates; consequently this quan-

tity of oxygen ought to be contained in the 143,76 of silex which the acid saturates, and the composition of silica should be

Silicium	48,025	100,00
Oxygen	51,975	108,22

This proportion comes very near to that obtained by synthesis, but it is difficult to say on which side the greatest errors of observation are to be found. According to what precedes, the weight of the atom of silicium, supposing that the silica contains three atoms of oxygen, would be 277,2, and, according to the best of the two synthetic experiments, 277,8. The other would carry it to 285; but this number by all appearance is too great. It exceeds by $1\frac{2}{3}$ per cent that which had been adopted at first; which is so well suited for the new and exact analyses of minerals, that if they were calculated according to the determination which we have just given, they would render an excess of silica necessary. But I must remark on this head, that a mineral is rarely found of which silica does not form an essential component, which does not yet contain from $\frac{1}{2}$ to 2 per cent of it, and even more, either in the state of quartz or in that of another siliceous mineral. This circumstance must take place so much the more with the minerals which have silica for one of their elements; and consequently all that quantity of it which is found above what the calculation gives should be attributed to a siliceous mixture existing in the mineral.

As to the number of atoms of oxygen contained in silica; the new facts do not furnish us with the means of determining this point. The circumstance that the carburetted silicium produces when burned an equal weight of silica, leads us to consider it as a quadricarburet, which by burning would give an oxide at three atoms of oxygen; but as I have not been able to isolate the carburet of silicium, nor to burn it completely, this result, although obtained in several experiments and with silicium produced by different operations, has not the certitude it ought to have, to be admitted as a proof. It ought notwithstanding to be considered as affording a strong presumption that silica contains three atoms of oxygen, so far as our knowledge respecting the crystalline form of bodies furnishes us with the means of deciding on the number of atoms of which oxides are formed. For the expression of the composition of the silicates by formulæ, it would certainly be more simple to consider silica as formed of an atom of silicium and an atom of oxygen; but it then would be diffi-

cult to conceive the existence of the silicates which contain six times the oxygen of the base, as in the apophyllite,—in which an atom of potash would then be combined with 12 atoms of silica.

To conclude: It still remains to be decided to what class of simple bodies silicium belongs. Since it has no lustre, nor the property of conducting electricity in the state in which it has hitherto been obtained, it is evident that it cannot be classed among the metals, and that its properties bring it near to boron and carbon. Some systematical philosophers will in consequence, doubtless, give it the name of *silicon*, to indicate by its termination the class of combustibles to which it should be referred. But I look upon this denomination as useless; for there is not any true limit between the metals and the metalloids. Carbon possesses the metallic lustre, and conducts electricity, but it is not considered as a metal; and if silicium could be melted, it then perhaps would have these properties, which it does not possess in a pulverulent state. Uranium, under this last form, can be distinguished but with difficulty by its appearance from silicium; and when crystallized, on the contrary, it has the metallic lustre and is transparent on the thinnest edges. Columbium and titanium also approach silicium by their chemical properties; and would it not be well to separate them from the metals, in order to unite them to the metalloids,—that is to say, to the non-metallic combustible bodies? I only wish to show, by these remarks, that there is no natural limit between these bodies; and that when their electric relation only is considered as exact, it is quite indifferent whether we place a combustible body among the metals or not.—*Annales des Physik and Chemie.*

ART. XII.—*Observations on the Habits of the Hyæna.* By ROBERT KNOX, M. D. F. R. S. E. Lecturer on Anatomy and Physiology, and Conservator of the Museum of the Royal College of Surgeons. Communicated by the Author. [*Jameson's Edin. Jour.*]

ABOUT a year ago, the Wernerian Society of Edinburgh did me the honor to publish in their Transactions, a brief notice by me of the habits of the hyæna, at present existing in Southern Africa, in which I endeavoured to show, 1st, That

the Rev. Mr Buckland, in his Geological Speculations, intended not so much to advance science, as to fix the era of a universal deluge, had taken up inaccurate, or at least by far too sweeping views of the natural habits of this ferocious animal; and 2d, That it was of little moment to a geological theory, what were the habits of modern or postdiluvian hyænas, since the antediluvian relics belonged to a different species; which latter circumstance Mr Buckland seems to have forgot, (or perhaps was not acquainted with,) until reminded of it by Dr Fleming. I shall now take the liberty of returning to the subject, and of pointing out to Mr Buckland, (since he has honoured my former brief notice with a short comment,) several facts in the history of these animals, which militate against his "Theory," and render his mode of accounting for the accumulation of bones in the Cave of Kirkdale, absolutely untenable.

1. In the immediate vicinity of the abodes of men, the hyæna usually drags his prey to a considerable distance, *in order that he may not be disturbed at his meal;* but his enormous voracity is a hindrance to his dragging the prey to any great distance, much less to store up food in caves, with a foresight exceeding most of the savage human tribes. Where the country is thinly inhabited, they will eat the carcase of an animal a few hundred yards from the houses; and where there are no houses. they will eat until they are filled to the mouth within thirty yards of a great number of armed men. I saw this happen;—a large striped hyæna was killed, during the night, by a sentinel, within thirty yards of an encampment of nine hundred men; on dissecting the animal, it was very evident that it had been engaged otherwise than dragging the bones to its den, (possibly twenty miles off,) for its stomach was so enormously distended with the offal of the cattle which had been felled all round the camp for the use of the troops, that it seemed, at first sight, to occupy the whole abdominal cavity. Now this dissection was performed by me before hundreds of persons.

2. I feel surprised that Mr Buckland can have forgot the account Bruce gives of a hyæna which ate, during the night, and close to his tent, more than would have sufficed half-a-dozen dogs. It is true, that Mr Bruce's reviewers made it out, that the hyæna had ate more than his own weight; but, setting aside this part of the story, which, of course, is a diverting exaggeration, Mr Bruce might have told his reviewers, that until they had examined the stomach of the hyæna,

they could not well imagine its vast capacity, commensurate with the natural voraciousness of the animal. I could easily multiply these instances from my own experience, for there were killed sometimes four or five hyænas within eight days, close to our habitations. Assuredly these animals were not busied in carrying away bones to remote caverns for future geologists to speculate upon.

3. It is not improbable that, in thickly inhabited countries the habits of the hyæna may be much altered, as we find to be the case in all other wild animals.* When much harassed, they become timed, and fly far from the abodes of men. I should be glad to offer this explanation in support of the supposed habits of the Kirkdale hyænas, but, unfortunately, the antediluvians had not discovered Britain, which, though a fine country, and full of nature's finest productions, did not boast of them as tenants.

4. Hyænas do not *congregate;* they are solitary. Consequently, all that Mr Buckland has said about *a den of hyænas*, is simply the work of the imagination heated by a false theory.

5. The young of the hyæna follow the dam early into the field, so that the quantity of food required to be carried to them must be small.

6. I do not believe that hyænas reside in caverns; they are too timid and distrustful of every thing. The almost inaccessible parts of the country to which they retire during the day-time, must have been visited by very few travellers. I cannot say that I have ever discovered hyænas in dens, though I have often been present when they were roused from their lurking-places.

These are all the observations I intend offering on this subject. It probably did not merit even these, since, as I formerly hinted,† the habits of modern and antediluvian hyænas might be entirely different, as they belong to different species. But however this may be, none who has inspected the Kirkdale bones, deposited in the Hunterian museum, can hesitate

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* My friend, Dr Versfield, a native of the Cape, informs me, that he has, on two occasions, observed caves in the Table Mountain, in the immediate vicinity of Cape Town, which contained a few bones, apparently of some ruminating animal; he ascribed the presence of these bones in the caves to the agency of hyænas, with which opinion I entirely agree. When we reflect on the proximity of so populous a town as the Cape to the Table Mountain, it is quite surprising that large collections of bones have not been found in its caves; the number of hyænas inhabiting these mountains must be very great.

† Wernerian Transactions, vol. v.

for a moment in declaring, that these bones have never been fractured by hyænas; they have been broken by great external violence, and not by the agency of the teeth of living animals; and they do not differ in any respect from the bones found at Oreston and elsewhere, which bear no such marks of violence. It seems indeed strange that the agency of wild animals should be resorted to in explaining the osteological collection of the Kirkdale cave, when so many other collections are allowed to have been formed in an entirely different manner. But the truth is, that we have evidence in the nature of the relics themselves, subversive of Mr Buckland's speculations on these subjects; 1. The bones found in the cave of Kirkdale do not bear the marks of having been broken by hyænas, but of having been dashed to pieces, and exposed to the action of water; 2. They belong to animals of a different species from any which now exist; like the mammoth, they have entirely disappeared from the face of the earth; the very race has been utterly destroyed. But the deluge which effected this, was not the universal deluge described in Scripture, for of all that then lived, and was liable to be destroyed by the devouring element, *individuals were carefully preserved to extend the race of animals to the present day.* Mr Buckland will, I trust, see the propriety of in future avoiding all speculations which have even a remote tendency to mingle up matters esteemed by most men as sacred, with short-sighted human theories, amongst which, I fear, must be ranked that which he has brought forward, and defended with much ingenuity and labour.

ART. XIII.—*Abstract of a Series of Papers lately read before the Royal Society, &c. on the Magnetism developed by Rotation.* By Messrs Barlow, Christie, Babbage, Herschell, and Marsh.* [*Philos. Mag.*]

On the Magnetism imparted to Iron Bodies by Rotation. By Peter Barlow, Esq. F.R.S.

THE anthor's attention having been recalled to the consid-

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*The great importance of the experiments and investigations detailed in these papers, has determined us to give in a connected form the official abstracts of them, as read before the Royal Society, from the Quarterly Journal of Science. We have added, to complete the series, Mr Marsh's account of Mr Barlow's repetition of M. Arago's late experiments, from the Edinburgh Philosophical Journal.

eration of the effects of rotation in altering the magnetic influence of iron, in the course of speculations on the cause of the rotation of the earth's magnetic poles, and knowing at the same time that Mr Christie had found a permanent change in the magnetic state of an iron plate by a mere change of position on its axis, it seemed to him highly probable that this change, due only to a simple inversion, would be increased by rapid rotation. On trial, however, it was found that the effect produced was merely temporary. The experiments at first were made with a thirteen-inch mortar-shell fixed to the mandrel of a powerful turning lathe, worked by a steam-engine in the royal arsenal at Woolwich.

This being made to revolve at the rate of 640 turns per minute, the needle was deflected out several degrees, and there remained stationary during the motion of the ball, but returned immediately to its original position on ceasing the rotation. On inverting the motion of the shell, an equal and contrary deflection took place.

As the law of the phenomena was not evident with this disposition of the apparatus, and the shell was found too heavy for perfect safety, a Shrapnell shell of eight inch diameter was mounted in a proper apparatus (described in the paper), and a number of experiments made, the law of which, however, still seemed anomalous, till the idea occurred of neutralizing the earth's action on the needle; when the anomalies disappeared, and the general law of the effect was placed in evidence. The needle being made a tangent to the ball, if the ball was made to revolve *towards* the needle (whatever was the direction of the axis of rotation), the north end of the latter was attracted, and if the contrary way, repelled. In the two extremities of the axis there was found no effect; while in two opposite points at right angles to the axis, the effect was a maximum, and the direction of the needle was to the centre of the ball.

The author then proceeded to show how the results, which before appeared anomalous, agree with this general view, and closed his communication with some theoretical views of their general bearing on the subjects of the earth's magnetism, which he thought there were strong reasons for believing to be of the *induced* kind; and although it appeared to him doubtful whether the anomalies observed in the variation of the needle on the earth's surface can ultimately be referred to this cause, yet he observed that one condition essential to the production of these phenomena holds good in

the case of the earth, viz. the non-coincidence of its polarized axis with that of its diurnal rotation.

On the Alteration in the Magnetism of an Iron Plate, occasioned by a Rotation on its Axis. By S. H. Christie, Esq.

The effects observed and described in this paper, although minute in themselves, appeared, in the author's opinion, to point out a species of magnetic action not hitherto described. It has long been well known that striking, twisting, or filing iron, in different directions, with regard to the magnetic axis, materially influences its polarity, but it does not appear to have been remarked that the simple rotation of iron in different directions has any such influence. This, however, the author has ascertained to be the case, and that the laws which govern this peculiar action are so regular, that there can remain no doubt of a corresponding regularity in their causes.

The attention of the author was first drawn to these phenomena by some apparent anomalies in the magnetic action of an iron plate on the compass, observed in the course of a different investigation. In order to avoid or allow for the disturbing influence of partial magnetism in the iron, it became necessary to attend minutely to the position of certain points in its circumference, which corresponded to the maxima and minima of this magnetism. It was then found that these points were not constant, but shifted their position as the plate was made to revolve in its own plane; or, in other words, that a plate which, in a given position, produced a certain deviation in a compass, no longer produced the same deviation after making an exact revolution in its own plane, although brought to rest, and every part of the apparatus restored precisely to its former place.

It appeared from this, that the revolution of the plate in its own plane had an influence on its power of deviating the needle independent of the partial magnetism of particular points in it; and the justice of this idea was proved by giving it a rotation in an opposite direction when the effect on its directive power was also reversed.

The change produced by rotation in the directive power of the plate was found to be a maximum when its plane was parallel to the line of dip on the magnetic axis, and at the same time as little inclined to the horizon as this condition would allow; but when the plane of the plate was parallel to

the horizon, the effect was diminished in the ratio of 5 to 1, and when perpendicular to the horizon, and coincident with the magnetic meridian, was altogether destroyed.

The author, having satisfied himself of the reality and constancy of this effect, in different plates, and of the necessity of referring it to a peculiar agency of the earth's magnetic power on the molecules of the plate, proceeded to ascertain the laws, and measure the quantities of the *deviation* due to rotation (so he terms it) in various positions; and detailed a great number of experiments, with their numerical results, arranged in the form of tables.

From these he deduced the following general law; viz. that the deviation due to rotation in *a dipping needle* "will always be such, that the sides of the equator of such dipping needle will deviate in a direction contrary to the directions in which the edge of the plate moves, that edge of the plate nearest to either edge of the equator producing the greatest effect."

The results of this law, it may be here observed, are in many cases coincident with those of the following: conceive the dipping needle orthographically projected on the plate. Then will the *deviation due to rotation* of the projected needle take *place in a direction opposite to that of the rotation itself.*

The author then proceeded to a theoretical investigation of the effect of a plate of soft iron, having within it two poles developed in given positions, and acting (in addition to the usual magnetic action of soft iron) on a needle of infinitely small dimensions, in the plane of the plate. He referred the whole ordinary action of the iron to its centre, and supposed that this is *attractive* on both poles of the needle; but the extraordinary action on that of the newly-developed poles he supposed to reside in them, and to be attractive or repulsive, according as they act on the poles of the needle of the same or opposite names with themselves. On this hypothesis, assuming symbols for the co-ordinates of the plate's centre, the distance separating the newly-developed poles in the plate, and the angle which the line joining them makes with the direction of the needle, &c., he deduced (from the known laws of magnetism) formulæ, expressing the horizontal deviations of the needle:—first, on the supposition of a rotation in one direction; secondly, on that of a rotation in the opposite; and thirdly, in that of no rotation at all. From these, by comparing them with a few of the observations, he deduced numerical values for the constants of the formulæ,

and then employed them to compute the deviations due to rotation in all the rest. He regarded the discrepancy between the calculated and observed results, as in few cases, larger than what he considered may be fairly attributed to error of observation; and that the theory above stated is at least a general representation of what passes in fact; admitting, however, that it does not give the exact position of the point where the deviation due to rotation vanishes, and suggesting partial magnetism in the iron plate used as one mode of accounting for the difference. At all events, by an examination of the case on the ordinary supposition of induced magnetism in the iron, he showed that a greater coincidence between theory and fact would not result from that hypothesis than from the one here employed.

He then proceeded to inquire into the degree of permanence of the polarity thus produced in iron by rotation; from which inquiry it appeared that (at least during 12 hours after the plate was brought to rest) the influence of a single rotation had scarcely suffered any diminution. It appeared also that the effect is so far from depending on the rapidity of the motion that the plate can scarcely be made to revolve so slowly as that the whole effect shall not be produced.

Lastly, by a slight change in the formulæ, the results of computation, it is found, can be made to agree with observation to a degree of exactness as near as can be wished. This change consists in the omission of certain terms introduced by the theory, and the author regards it as very possible so to modify the theory as to get rid of them.

The author closed this communication with an appendix comparing the magnetic effects produced by slow and rapid rotation. The result of the comparison was, that the forces exerted on the needle during rapid rotation were always in the same direction as those derived from the slowest rotation, and which continue to act after the rotation has ceased, but were greater in intensity, and that the former effects were such as might have been looked for from a knowledge of the latter.

An Account of the Repetition of M. Arago's Experiments on the Magnetism developed during the Act of Rotation. By Charles Babbage, Esq. F.R.S., and J. F. Herschell, Esq. Sec. R.S.

The experiments of M. Arago having excited much interest, the authors of this communication were induced to erect

an apparatus for their verification; and after a few trials, they succeeded in causing a compass to deviate from the magnetic meridian, by setting in rotation under it plates of copper, zinc, lead, &c.

To obtain more visible and regular effects, however, they found it necessary to reverse the experiment, by setting in rotation a powerful horse-shoe magnet, and suspending over it the various metals, and other substances to be examined, which were found to follow with various degrees of readiness the motion of the magnet. The substances in which they succeeded in developing signs of magnetism were, copper, zinc, silver, tin, lead, antimony, mercury, gold, bismuth, and carbon in that peculiar metalloidal state in which it is precipitated from carburetted hydrogen in gas works. In the case of mercury, the rigorous absence of iron was secured. In other bodies, such as sulphuric acid, rosin, glass, and other non-conductors, or imperfect conductors of electricity, no positive evidence of magnetism was obtained.

The comparative intensities of action of these bodies were next numerically determined by two different methods, viz., by observing the deviation of the compass over revolving plates of great size cast to one pattern, and by the times of rotation of a neutralized system of magnets suspended over them; and it is curious that the two methods, though they assigned the same order to the remaining bodies, uniformly gave opposite results in the cases of zinc and copper, placing them constantly above or below each other according to the mode of observation employed.

Our authors next investigated the effect of solution of continuity on the various metals; in the course of which M. Arago's results of the diminution of effect by division of the metallic plates used were verified; and the further curious fact ascertained, that re-establishing the metallic contact with other metals restores the force, either wholly or in great measure; and that even when the metal used for soldering has, in itself, but a very feeble magnetic power, thus affording a power of magnifying weak degrees of magnetism. The law of diminution of the force by increase of distance was next investigated. It appears to follow no constant progression according to a fixed power of the distance, but to vary between the square and the cube.

The remainder of this paper was devoted to some able and elaborate reasoning on the facts detailed.—The authors conceive that they may be all explained without any new hypo-

thesis in magnetism, by supposing simply that time is requisite both for the developement and loss of magnetism; and that different metals differ in respect, not only of the time they require, but in the intensity of the force ultimately producible in them; and they apply this explanation not only to their own results, but to those obtained by Mr Barlow in his paper on the rotation of iron.

Experiments on the Magnetism produced by Rotation. By S. H. Christie, Esq., in a Letter to Mr Herschel.

Mr Christie, in this communication, gave an account of some experiments on the developement of magnetism in copper by rotation. He corroborated by his own experience the results obtained by Mr Herschel, in which a disc of copper was set in rotation by the rotation of one or more magnets beneath it, both in the case where poles of the same name were immediately below the disc, and when of a contrary name. The actions appeared equally intense in both cases; and from this circumstance, he concludes the magnetism thus communicated to the copper to be extremely transient. The experiment was varied by combining the revolving magnets differently, and the results were stated.

The next experiments of Mr Christie were directed to the determination of the law according to which the force diminishes as the distance between the disc and magnets increases. It seems to follow from these experiments, that when a thick copper plate is made to revolve under a small magnet, the force tending to deviate the needle is directly as the velocity, and inversely as the fourth power of the distance; but that when magnets of considerable size are made to revolve under thin copper discs, the diminution follows more nearly the ratio of the inverse square of the distance, or between the square and the cube, though not in any constant ratio of an exact power.

The author then investigated the law of force when copper discs of different weights are set in rotation, which, for small distances, appear proportioned to the weights of the discs, but for smaller ones appear to vary in some higher ratio.

Account of the Repetition of M. Arago's Experiments on the Magnetism developed during the Act of Rotation. By Messrs Barlow and Marsh.

The experiments by Mr Barlow on the magnetism imparted

by rotation, described in p. 124, were begun in Dec. 1824; and it was not until April 1825 that he was informed of M. Arago's rotative experiments on copper and other metals. "The latter were not known in England." Mr Marsh states, "until M. Gay-Lussac's visit to London at the time above stated."—"I am not aware," he continues, "of the precise nature of these experiments; and shall, therefore, only endeavour to describe those which I have assisted Mr Barlow in making, and which he founded on the description he had received; they may, therefore, be considered as the experiments of M. Arago repeated, and varied as different circumstances occurred to suggest new ideas. The account he had of M. Arago's experiment, was that, by placing a copper plate upon a vertical spindle, the plate being horizontal, and then placing just above it a light compass needle, but independent, of course, of the plate; on causing the spindle and plate to revolve, the needle was considerably deflected, and more and more as the velocity was increased; so that, when the plate was put into rapid rotation, the needle also began, after a few vibrations, to revolve, and at length with considerable velocity.

"1. In order to repeat this experiment, I connected the wheel of my turning lathe with a vertical spindle, which I could make revolve forty-five times per second; and on this I placed a thin copper plate, about six inches in diameter, and over this a needle about five inches long, shut up in a close box, about one inch, or rather less, above the plate. When putting the lathe in motion, I found it to deflect the needle about five points, the deflection being always in the same direction as the motion of the plate, but we could not cause it to revolve. The needle was, therefore, partly neutralized by a bar magnet, and the experiment repeated. We then very soon obtained a considerable rotatory motion in the needle; and, by using a larger and heavier plate, the same was produced afterwards without neutralizing the needle.

"2. Another experiment, which was mentioned as one of M. Arago's, and which I repeated, was, by interposing a plate of iron between the copper plate and the needle. In this case, no effect could be produced on the needle by the rotation of the copper plate, the iron clearly intercepting the action.

"3. The only other experiment that I am aware of as originating with M. Arago, at least that I repeated, was the rota-

tion of a plate cut into radii like a star, which was said, as I understood, to produce no effect; this, however, was not the case in my experiments,—it certainly produced a less effect, but, I think, not less than might have been anticipated, from the quantity of copper thus taken away.

"4. I now tried a zinc plate instead of a copper plate, and the effect was nearly the same as before, but a little less.

"5. An iron plate was now substituted, and the effect was considerably greater than with the copper plate.

"6. The copper plate was again replaced, and a brass needle placed in the box. Some motion was obtained, but it was very equivocal, so that I cannot venture to say that it was certainly due to the rotation.

"7. A heavy horse-shoe magnet was now suspended by a line from the ceiling; and it was put in rotation by the revolution of the copper plate, a paper screen having been first interposed between them.

"8. One copper plate was suspended over another, but no motion was obtained; and the same took place when the copper plate was suspended over an iron one.

"9. A bar magnet, rather shorter than the diameter of the copper plate, was fixed horizontally to the upright spindle; and being made to revolve, the plate very soon acquired rotation. A paper screen was, in this, as in the preceding experiments, interposed between the plate and magnet.

"10. The plate was now applied immediately to the axis of the lathe, so as to cause it to revolve vertically, and the needle placed near to it; but no motion took place, till, by nearly neutralizing the needle, and bringing either of its poles directly to the plate, it then always deviated in the direction of the motion of the plate; whichever pole of the needle was directed to the former. The needle, of course, therefore, deviated different ways (all other things being the same), when it was above or below the axis; but in the direct horizontal line of the axis no motion in the needle took place.

"11. The above are the principal experiments that I assisted in making by revolving the plate; but these having suggested to Mr Barlow that all the results obtained might be explained, by supposing that there existed a slight magnetic power in copper, and in the various metals which had a tendency to draw the needle after the plate, or the latter after the former, he endeavoured to exhibit this by direct experiment, independent of revolution. With this view, he neutral-

ized a needle very accurately; and then applying very near to its poles the end of a round brass ruler, the attraction of the latter was obvious,—it drew the needle several degrees,—then, withdrawing it, and catching the needle again in its returning vibration, it was drawn out some further degrees; and, in a very short time, the deflection was converted into a revolution, which, by alternately presenting and withdrawing the needle, was at length rendered very rapid.

"12. The same result was obtained by two or three different pieces of brass; but there were other pieces, although of the same size and form, which had little or no effect.

"The following experiment is due to Mr Sturgeon, of Woolwich.

"13. A thin copper plate or wheel, about five or six inches in diameter, was suspended very delicately on an axis, and then one side a little weighted, in order to give it a tendency to oscillate. The heavy point was now raised level with the axis, and the number of vibrations the plate made before it came to rest were counted. The same was again done, with this difference only, that the vibrations now took place between the poles of a horse-shoe magnet; and the number of them before the plate came to rest, was very little more than one half of what they were in the former instance.

"This is the converse of M. Arago's experiments, in which he shows the effect of copper and other metallic rings, in diminishing the number of oscillations of a magnetic needle.

"14. If, instead of a horseshoe magnet, the contrary poles of two bar magnets be used, the effect is the same as before; but, if the poles of the same name, viz. both north or both south, be employed, then the effect is scarcely perceptible. This is an important result, as it shows that the effect is not due to any kind of resisting medium, as was supposed in the first instance."

ART. XIV.—*Researches on a New Acid universally diffused through Vegetables.* By M. H. Biaconnet. (*Philos. Jour.*)

I first obtained this acid from the tubercles of the dahlia and artichoke, whilst engaged in their analysis, but ignorant of most of its properties, I neglected to insert it as a con-

stituent principle. Some time after, whilst examining the roots of cultivated celery, I found the same principle, and observed its acid properties. It has, since then, occurred so frequently, that I have not met with a single vegetable or succulent root without observing its presence.

I have found it in the roots of the turnip, carrot, phytalacca, (phytalacca decandria,) scorzonere hispanica?) piony, phlomede (tubereuse) in the roots of patience (Rumex patienza,) and of filipen dule (saxifraga rubra) where it is united to a colouring principle, in bulbs, the onion; in the stalks and leaves of herbaceous plants; in the cortical layers of all trees previously deprived of the coloured external bark, from whence it may be obtained in great abundance, sometimes united to a red colouring matter, as in the cherry-tree, maple, nut-tree, at other times colourless nearly, as in the elder tree; in sawdust of the maple; in apples, pears, prunes, cucurbitaceous fruits, and, without doubt, in all other fruits, and in grain.

Before describing the properties of this acid, I should state, that it appears to me very analogous to, if not identical with, the principle so little known under the vague term of jelly. However that may be, it is easily obtained from all parts of plants. If roots containing starch be operated upon, such as those of celery and carrot, they are to be reduced to pulp by rasping, the juice expressed, the residue boiled with water, slightly acidified with muriatic acid, then washed, and afterwards heated with a very dilute solution of potash or soda; a thick mucilaginous liquid results, slightly alkaline, from which muriatic acid separates the new acid in the form of an abundant jelly, which should then be well washed. In this state it is almost colourless, especially when from colourless vegetable substances; it has a sensibly acid taste, and applied to litmus paper, distinctly reddens it, though containing no foreign acid.

It is scarcely soluble in cold water, but a solution may be obtained, sensible to re-agents. Boiling water has a stronger action; the filtered liquor is colourless, deposits nothing on cooling, and barely reddens litmus; alcohol coagulates it into a transparent jelly, colourless as ice; so also do all metallic solutions, lime-water, baryta-water, acids, muriate and sulphate of soda, nitrate of potash, &c. This acid appears to be held so feebly in aqueous solution, that the solution of a little sugar is sufficient to make the greatest part of the liquor coagulate. Dried in a capsule, it appears in transpar-

ent films having no adhesion to the vessel. When dry, it scarcely swells in cold water; dissolves in small quantities in boiling water, and offers the phenomena just described. The acid precipitated from its potash combination by muriatic acid, when distilled did not swell up, and gave a product containing much empyrumatic oil, but no ammonia nor muriatic acid; much charcoal remained. Diffused through water, and aided by warmth, it disengages carbonic acid from its alkaline combinations.

It forms a very soluble salt with potash, which may be obtained in the state of transparent jelly, by adding weak alcohol, which removes the excess of alkali, and colouring matter, if there be any. This jelly, washed on a cloth with alcoholized water, pressed, and dried, is a neutral combination, which swells and dissolves in water, and leaves upon evaporation of the liquid a transparent mass, full of cracks, and resembling gum arabic in appearance; it has so little disposition to adhere, that the slightest friction removes it entirely from the capsule. The taste of this salt is insipid; put upon a plate of red-hot iron, it swells excessively, leaving a deep-brown residue, soluble in water, and having the character of ulmin, united to potash. Exposed to the flame of a taper, it burns, producing delicate filaments, which project out from the mass like vermicelli. These filaments fuse into globules of sub-carbonate of potash.

This salt, in aqueous solution, is gelatinized by alcohol, sugar, muriate of soda, acetate of potash, and other neutral salts. All earthy and metallic salts decompose it by double affinity. The acids unite to the alkali, and separate the acid as a jelly. One hundred parts of the neutral combination burnt in a platina crucible, left a quantity of sub-carbonate of potash, which, heated to redness with sulphuric acid, gave 28 parts of sulphate of potash, from which it would appear, that the salt was formed of

Acid	85
Potash	15
	100

Let it not be supposed that this salt will be useless; I am persuaded that it will have many applications in the art of the confectioner. It is, indeed, remarkable, that so small a quantity of the combination can communicate the property of gelatinizing to such large quantities of sugared water. I dissolved one part of this salt obtained from the turnip root

in a certain quantity of warm water; sugar was then dissolved in the liquor, and finally a very small quantity of acid added; an instant afterwards, the whole became a trembling gelatinous mass, weighing 300 parts. I have in this prepared aromatic jellies, perfectly transparent and colourless, and very agreeable to the taste and sight. By colouring rose-water with a little cochineal, I have also prepared jelly of roses, of an exquisite taste.

The same acid added to a weak solution of ammonia, forms also a soluble compound, which evaporated to dryness, leaves a neutral insipid salt, something like gum, which separates from the evaporating dish in large plates, like mica. Its solution coagulates with different substances in the same manner as the preceding. I dissolved one part of this combination in 100 parts of water, and added alcohol, which separated a transparent jelly; this, drained on a cloth, weighed 110 parts. This salt will answer like the preceding in the preparation of jellies; and indeed is more advantageous since it may be readily obtained in the neutral state.

The other combinations of this acid are almost all insoluble and may be obtained by double decomposition.

Concentrated sulphuric acid appears to have but little action when cold on this acid; by means of heat it produces sulphurous acid and ulmin. Nitric acid distilled to dryness with it leaves a residue, which with water yielded oxalic acid and a white powder, which was partly dissolved by ammonia, and left oxalate of lime. An acid added to the ammoniacal solution separated a granular sediment, crystalline acid, and having the character of mucous acid; heated in a glass tube, it blackened, fused, swelled up, and produced acicular crystalline sublimate.

Such are the principle properties which I have observed in this acid. It may be presumed that a principle thus universally spread over all vegetables, has an important part to play, and merits the serious attention of physiologists. I am very much disposed to think that it is the same thing as the *cambium* or organizable substance of Gren and Duhamel, and which appears, as is known, in gelatinous drops. Wherever new developements are forming, I shall endeavour in the spring to ascertain whether my conjecture is well-founded. In the mean time, I propose the name of *pectic acid*, from πηκτις (*pēctis*) coagulum, to distinguish it from other bodies of the same class.

ART. XV.—*On some Kinds of Fulminating Powder inflammable by Percussion, and their Use in Fire-arms.* By P. W. Schmidt, Lieutenant in the Prussian service.* (*Philos. Mag.*)

A POWDER inflammable by percussion, has been used for some years past, especially in fowling-pieces. The following formulæ have been given for the preparation of this powder, the principal ingredient of which is chlorate of potash.

1. 100 parts of chlorate of potash (fulminating salt,) 12 parts of sulphur, and 10 parts of charcoal are closely mixed. The grains are produced by forcing the soft paste through a sieve.

2. 100 parts of chlorate of potash, 42 parts of saltpetre, 36 parts of sulphur, and 14 parts of lycopodium.

These are the usual ingredients that have hitherto been mixed with the chlorate of potash for the purpose of making priming powder. The guns, however, with which this powder is used, are very various in their construction. In some it explodes of itself by means of the mechanism of the lock, on being cocked, into a small conical recess, which communicates with the touch-hole, in others, it is put in previous to every shot. In the former kind of guns a quantity of powder sufficient for a certain number of shots is kept in a recess attached to the lock, called a magazine; and the locks (which were invented in England by Mr Forsyth) are called maginine locks.

In some guns the stroke of the cock, which is in the shape of a hammer, falls immediately on the fulminating powder strewed in the above recess. In order to protect the powder from wet, small balls of it were covered over with wax, and placed sometimes in the conical recess, and at others fixed to the cock itself. In both instances the ball was kindled in the recess just mentioned. by means of the percussion.

Besides these, other contrivances have been used for the purpose of igniting this kind of powder; yet they have all their defects, and offer so many difficulties in practice as to have prevented their general introduction.

Latterly, they have contrived in Germany to fix the powder in a small case of very thin copper foil, for the purpose of keeping it dry; and for that purpose a cylinder is screwed into the body of the gun instead of the touch-hole, and

* From Schweigger's Journal, Band xi. p. 66.

rests for the sake of greater support on the plate of the lock, instead of resting on the pan. The inner space of the cylinder is filled in loading with the same powder as that of the shot. The igniting box, at the bottom of which is the detonating powder, is, previous to firing, upturned on the cylinder. In this cylinder is a small round aperture leading to the inner space of the cylinder. On the trigger being pulled, the cock strikes the igniting-box, and the fulminating powder is kindled by the blow, flows through the aperture, inflames the shot, and breaks the igniting-box.

Mr Wright seems to have taken great pains with the subject.* He recommends, for the firing cases, to use fulminating mercury, saying that sportsmen had justly complained of the powder made of chlorate of potash, since it soon produces the oxidation of the barrel and touch-hole, and the charcoal which remains after the firing, rendering them unfit for use. The advantages of his new powder he enumerates as follows: It does not make the gun rusty so soon as the other; it produces neither dirt nor moisture; it is not liable to explode as the other powder, and if it does explode its effects are less destructive, inasmuch as its power does not extend so far.

The following is his mode of preparation:

"I place two drachms of quicksilver in a Florence flask, and pour six drachms (measure) of *pure* nitric acid on the mercury; this I place in a stand over a spirit-lamp, and make it boil till the quicksilver is taken up by the acid; when nearly cool, I pour it on an ounce (measure) of alcohol in another flask: sometimes immediate effervescence ensues, with the extrication of nitrous æther; and often I have been obliged to place the mixture over the lamp, till a white fume begins to rise, when the effervescence follows. I suffer the process to continue (removing the lamp) till the fumes assume a reddish hue; when I pour water into the flask, and the powder is found precipitated to the bottom, I pour off and add fresh water, permitting the powder to subside each time before the water is poured off, so as to free the substance as much as possible from the acid, and then I pour it on a piece of filtering paper, and place the powder in an airy room to dry. It should be kept in a corked (not stoppered) bottle."

For the filling of the cups, he makes use of an ivory pin, which has a scoop at one end for the purpose of receiving

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* Mr Wright's paper will be found in Phil. Mag. vol. lxii, p. 203.

the powder, and at the other is cut flat. With it he puts in as much fulminating mercury as will cover the bottom; he then dips the flat end into a strong tincture of benzoin, and rubs this substance gently about the case; by which means the powder is set fast and covered as with a varnish.

Professor Schweigger, speaking of these kinds of experiments in his chemical lectures, observed on the practicability of kindling gunpowder by the admixture of other substances, as has been shown in a criminal investigation that took place at Munich a few years ago. A box filled with gunpowder was sent to an individual provided with fulminating papers, which were to inflame on the box being opened. Fortunately, however, the murderous design was frustrated; for although the papers exploded, they did not kindle the powder. The assassin was discovered and punished. M. Gehlen, who had been examined at the trial, was led by the circumstance to make several experiments for the purpose of kindling gunpowder by means of Brugnatelli's fulminating silver, but they all failed.

It seems that in England, too, difficulties had been found in igniting gunpowder with fulminating mercury; for Mr Wright observes, "if any one doubts the practicability of firing gunpowder by means of fulminating mercury, by procuring a percussion-gun, he may try the experiment and be fully satisfied."

Professor Schweigger having, therefore, requested me to try some experiments on this subject, especially with fulminating silver, I made them in the chemical laboratory of our university, and the following were the results.

I. Fulminating silver was prepared in the usual way; five drachms of fuming nitric acid, and five of alcohol were poured over one drachm of fused nitrate of silver. When the effervescence and solution were complete, water was added. The precipitate of fulminating silver was filtered off, and all the remaining acidity washed from it with water. The liquid which had passed through the filter gave with muriatic acid a copious precipitate of chloride of silver. The fulminating silver, which was of a whitish tint, was now subjected to the following experiments:

1. When damp it ignited very rarely, and only by a strong blow. When dry, it explodes easily, and with a much slighter blow.

2. When touched with sulphuric acid, it exploded equally strongly, whether damp or dry.

3. Damp or dry, it exploded in the fire.

4. The substance which remained after the ignition, was of a bluish brilliant hue and a disagreeable metallic taste. I could gather but little, which, dissolved in water, produced a faint red tint on litmus paper.

5. I failed in several attempts to ignite gun-powder with the fulminating silver. I therefore put quantities of the size of a small pin's head, into some copper boxes, fastening it in some with tincture of benzoin, in others with a solution of gum arabic in water; and others I tried to press on the bottom without any other aid. I applied them to guns prepared for the use of igniting-boxes, and thus kindled the gun-powder with incredible swiftness. The series of experiments thus made in the presence of Professor Schweigger, leave no doubt that fulminating silver will easily ignite gun-powder in a mannner that will secure it against a rapid dispersion on exploding.

II. The fulminating mercury was prepared in the manner prescribed by Mr Wright. But I must observe that the experiment only succeeded by the application of fuming nitric acid. The fulminating mercury, when obtained, was washed till every particle of acidity had disappeared from it. It was then submitted to the following experiments:

1. When dry, it exploded like fulminating silver, and with a much slighter blow than required for the powder made of chlorate of potash. Thus it would appear as if the fulminating quicksilver had not in this respect the advantage over the igniting powder made with the salt just named.

2. When perfectly dry only, it could be ignited with sulphuric acid.

3. In the fire it exploded, both wet and dry.

4. The substance remaining after the explosion, had a bluish brilliant tint and a bitter acid metallic taste. With a small quantity dissolved in water, litmus paper was slightly reddened. I made no further investigation, inasmuch as the examination of the acids of fulminating metals, before and after the explosion, was not my object; especially since Dr Liebig has lately published a series of very interesting experiments, the repetition of which would require very extensive labour.* Liebig calls those acids fulminic acids, which

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* Vide *Ann. de Chim. et de Phys.* part xxiv. p. 294, or the translation in Gilbert's *Annalen der Phys.* part lxxv. p 393—422. Mr Liebig found that Brugnatelli's detonating silver was dissolved in lime-water or solutions of the caustic alkalies, whereby

being the property of all metals, he distinguishes into silver and mercury fulminic acids, &c.

The great advantages, however, of the fulminating quicksilver as igniting powder, extolled by Mr Wright, I did not find confirmed; although I proceeded to fill copper boxes, as I had done with the detonating silver, which all ignited the charge.

III. I also submitted to experiment the first-named mixture, principally consisting of chlorate of potash; and found

1. That it exploded only by a hard blow. Its effects were much less than those of the detonating silver or quicksilver. Mr Wright, indeed, says the contrary of the latter; it seems, therefore, that I used a better kind of fulminating mercury; but for that very reason I must give the chlorate of potash the preference for practical use.

2. In the fire it puffed away like gunpowder.

3. The substance remaining after the explosion is blackish and dusty, and contains less of acidity than that left by the quicksilver. Thus, and indeed from all my experiments, it is evident that it oxidizes the iron less than the fulminating mercury. Moisture is also left by the latter; and the charcoal left by the mixture of chlorate of potash, after kindling a copper box filled with it, is very unimportant:—therefore this mixture is preferable as an igniting powder. This is also the reason why the manufacturers no longer use the fulminating quicksilver. I know one who makes and fills weekly several thousands of copper caps, for which he uses the chlorate

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31,25 per cent of oxide of silver was deposited. They produce peculiar salts, called *fulminates*, which explode with great violence. These salts are dissolved by nitric acid, sulphuric acid, and acetic acid; the silver fulminic acid contained in them, and so difficult to be dissolved, is deposited; and by heating a solution of fulminate of lime to the boiling point, and adding a moderate quantity of nitric, acid is deposited, on cooling, at the bottom of the vessel, in the shape of long white crystals. This acid may be easily dissolved in boiling water; from which it crystallizes again in cooling, has a disgusting metallic taste, and reddens litmus paper. But it cannot subsist of itself without combination with a metal; and in the same manner as there are prussic acids of iron, copper, silver, and gold, so the fulminic acid combines with silver, quicksilver, copper, iron, zinc, &c., into proper fulminic acid, which again form different combinations with the bases, *e. g.* potash, soda, barytes, strontian, lime, &c. Thus, for instance, silver-fulminate of potash consists of 35,03 parts of silver-fulminic acid and 14,92 of potash; silver-fulminate of soda, of 38,66 parts of silver-fulminic acid and 11,34 of alkali. When cooling, Berthollet's detonating silver forms granular shining white crystals. One part of this salt makes as violent a report as three parts of Howard's (Brugnatelli's) fulminating silver. With magnesia the silver-fulminic acid combines in two ways. One combination is a simple decrepitating, not detonating, insoluble powder, of a rosy tint; the other forms white capillary crystals, and explodes very loudly. The first combination was used for the analysis of fulmic acid in the dry way; in which the fulminating silver was evinced to consist of 32,22 of oxygen, 3,22 of hydrogen, 11,28 of azote, 9,68 of carbon, and 41 of silver.

mixture, the preparation of which is both less expensive and less dangerous than that of the fulminating quicksilver. There is another circumstance attending this mixture. In filling the caps, it will sometimes happen that the quantity put in is doubled, which I find is of no injurious consequence with this mixture; but might endanger the person firing with the fulminating mercury, as the box will burst too violently.

With respect to the power of igniting the charge, the different kinds of powder which I have compared are equally effectual.

IV. I submitted the mixture of chlorate of potash mentioned above to the following experiments.

1. That part only ignited which was struck, without igniting that lying around it.

2. In the fire it burns away with noise.

3. I placed it in the usual way in copper caps, but could not ignite a charge with them. The cause of this may be explained by the construction of the locks, with reference to the properties of this detonating powder. That part of the cap situated just above the opening of the cylinder remains, as the blow cannot fall on it unignited, as shown by the experiment No. 1. But the communication of the ignited part with the charge, is prevented by the manner in which the cock strikes the cap. In guns in which such powder is used for igniting, it lies as above stated, in small balls in a conical aperture. Here it is nearly all ignited by the striking of the cock, and must of necessity flow inwards, every other way of escape being shut up.

In conclusion:—I have to add that the method of filling the caps recommended by Mr Wright is not only laborious, but even dangerous. How are manufacturers to employ that method when they have to fill several thousands a week? I have made various trials, and the following process seems to me to be the best.

Pour some adhesive solution or tincture over the powder, and mix it into a stiff kind of liquid. Take with a brush or a stick a large drop of it, and apply it against the bottom of the cap.

This method is both quick and free from danger; whilst on filling with the dry detonating powder, the least careless touch may produce an explosion.

In order to prevent the corrosion of the cylinder, and its becoming useless by the formation of sulphuret of iron (an evil very common with iron touch-holes, and caused more by

the action of the gun-powder than by that of the igniting substance), the inside of the cylinder should be lined with a metal which will neither oxidate nor easily combine with the ingredients of the powder.

ART. XVI.—*On the Analysis of Atmospheric Air by Hydrogen.* By John Dalton, Esq. F. R. S. &c. (*Ann. Philos.*)

ACCORDING to my promise, I transmit the results of some late experiments on the analysis of atmospheric air by hydrogen. My chief object was to find under what circumstances the union of the oxygen and hydrogen, by the electric spark, is complete, that is, so that one or both of the gases are entirely consumed; and in what cases either no union takes place or a partial one, leaving portions of both gases still in mixture in the residue.

From a memoir of MM. Humboldt and Gay-Lussac (*Ann. de Chimie*, 53, 1805,) we learn that one volume of hydrogen, mixed with two or nine volumes of oxygen, gives the same loss by electricity, namely, 1,46; but if mixed with 9,5 oxygen, the loss is only ,68; and this loss diminishes rapidly, till the oxygen becomes 16, when there is no loss at all. They found that if the surplus gas was azote or carbonic acid, the loss was not much different; but they do not seem to have ascertained this with precision.

It is right to observe that the hydrogen I used was obtained in the usual way from zinc and diluted sulphuric acid, and was received in bottles filled with as pure rain-water as I could procure; the bottles were filled with the gas, and not more than one third of the gas of each bottle was used; the hydrogen was free from atmospheric air, except what was expelled from the water by the hydrogen bubbling into the bottle: this quantity of atmospheric air, however, must be something; yet on firing 10 measures of hydrogen with oxygen, the diminution is usually 14,6 to 15.

The mixtures of gases fired at once were commonly about 150 measures, each measure being the volume of one grain of water. The eudiometer has six inches in length correspondent to 150 measures, and all the experiments were made over water.

The atmospheric air I mostly used was procured in the

country, and was found by frequent trials to contain almost exactly 21 per cent. of oxygen. This is not the case at all times. I once found the oxygen as high as 21,15 per cent. from an average of many experiments; it was on the 8th of January last, when the barometer was 30,9, wind N.E. and very moderate, after three days of calm and gentle frost. But the general state of the atmosphere yields only 20,7 or 20,8 per cent. of oxygen. All the results below must be considered as averages of four or five experiments.

Tabular Arrangement of the Experiments.

Measures of Oxygen.	Measures of Mixtures	Azote.	Hydr.		Measures.	
1	to 14,0	= (3,76	+ 10,24)	Does not explode		
1	13.0	= (3,76	+ 9,24)	Fires imperfect	Loss 2,7....0	& h
1	12,0	= (3,76	+ 8,24)	" perfect	" 3,0....0	h
1	11,0	= (3,76	+ 7,24)	"	" 3,0....0	h
1	10,0	= (3,76	+ 6,24)	"	" 3,0....0	h
1	9,0	= (3,76	+ 5,24)	"	" 3,0....0	h
1	8,0	= (3,76	+ 4,24)	"	" 3,0....0	h
1	7,0	= (3,76	+ 3,24)	"	" 3,0....0	h
1	6,0	= (3,76	+ 2,24)	"	" 3,0....0	h
1	5,86	= (3,76	+ 2,10)	"	" 3,0....0	n
1	5,76	= (3,76	+ 2,00)	"	" 3,0....0	o
Hydr.			Oxg.			
or 1	2,38	= (1,88	+ 0,5)	"	" 1½....	o
1	3,00	= (2,37	+ 0,63)	"	" 1½....	o
1	4,00	= (3,16	+ 0,84)	"	" 1½....	o
1	5,00	= (3,95	+ 1,05)	"	" 1½....	o
1	6,00	= (4,74	+ 1,26)	" rather imper.	" 1½....	o
1	7,00	= (5,53	+ 1,47)	" imperfect	" 1,0....0	h
1	8,00	= (6,32	+ 1,68)	"	" 1,0....0	h
1	9,00	= (7,11	+ 1,89)	"	" 0,75....0	h
1	10,00	= (7,90	+ 2,10)	"	" 0,60....0	h
1	11,00	= (8,69	+ 2,31)	"	" 0,45....0	h
1	12,00	= (9,48	+ 2,52)	"	" 0,30....0	h
1	13,00	= (10,27	+ 2,73)	"	" 0,20....0	h
1	14,00	= (11,06	+ 2,94)	Does not fire sometimes; fires at others.	" 0,20....0	h

N. B. Those experiments marked *h* denote that hydrogen remained in the residue after detonation; those marked *o* denote oxygen; and that marked *n* denotes that neither of the two was found.

On this table it may be remarked; 1. The rapid transition from no detonation to a perfect one, when the oxygen is near a minimum; 2. The slow transition at the bottom of the table when the hydrogen is near the minimum; and 3. That there is no imperfect combustion about the middle of the table; either the oxygen or the hydrogen, or both are always entirely gone. But there is one anomaly that calls for explanation,—the neutralizing proportions of oxygen and hydrogen appear to be 1 to 2,1 when it is well known that they are nearly, if not exactly, as 1 to 2. This is occasioned, no doubt, by the impurity produced in the hydrogen; first, in the reception of it in bottles with water charged with atmospheric air; and, secondly, in the subsequent passing of it two or three times through water in the process for detonation. That this is the true reason, is confirmed by the loss being only 3, and not 3,1 in that case; and by the subsequent proof that more azote exists in the residue than ought to do, on the supposition the hydrogen was quite pure.

Such persons as are not familiar with this kind of experiment, and wish to repeat any of them, will do well to remember that whenever oxygen and hydrogen are mixed in *nearly the saturating proportions*, the mixture should stand some time (five minutes), in order to allow of the perfect diffusion of the gases, before the spark is given.

Art. XIV.—*On the Aurora Borealis.**

The aurora borealis appears in the night at irregular intervals, extending itself along the northern part of the heavens, now as an indefinite faint light, rising a little above the horizon and resembling the twilight; now as phosphoric coruscations, suddenly traversing and illuminating the whole atmosphere. These luminous appearances were for a long time the only circumstance that engaged the attention; but in 1740, two Swedish observers, Celsius and Hiorter, discoved other and entirely new phenomena in this meteor, which being intimately connected with its nature, very much ex-

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* This article is, with the permission of Prof. Farrar, taken from "A Treatise on Electricity, Magnetism, and Electro-Magnetism, being the second part of a Course of Natural Philosophy, compiled for the use of the Students of the University at Cambridge, N. E."

tended the views which had been previously entertained upon this subject. They observed, that during the appearance of the aurora borealis, magnetic needles, freely suspended, almost always undergo very irregular agitations, which needles not magnetic, those of copper, for instance, do not exhibit. If we compare observations of this kind made at places very distant from each other, as at Upsal and London, for instance, we find that the motions are the same. It appears, also, that their violence depends on the brightness and extent of the aurora borealis. A low and faint glimmering, towards the northern horizon ordinarily produces only a very slight, and perhaps insensible, disturbance of the magnetic needle. Moreover, the motion is very slight in the case of an elevated meteor when the principal focus is situated in the plane of the needle's direction, usually called the plane of the magnetic meridian. We remark further, that when the phosphoric jets are numerous, the atmosphere at the same time being calm, or only agitated by a steady breeze, we almost always observe that the substance of the meteor is disposed in one or several concentric arcs, resembling those of the rainbow, now white, and now tinged with the brightest colours. But we almost always find, that the common centre of these arcs and their summits are situated in the magnetic meridian of the place where they are observed, so that they are all similarly situated with respect to this plane: and this coincidence with the meridian, which still exists, has been remarked ever since any accurate observations were made, although during this time there have been very considerable variations in the direction of the magnetic meridians in Europe; so that the mean direction of the meteor in the horizon of each place, has also undergone an equal change. Furthermore, it sometimes happens, that the phosphoric fires, breaking forth from all parts of the horizon, from the east, the west, and the north, ascend, or seem to ascend, vertically over the head of the observer, even to his zenith, and having passed this point, they form by their union a brilliant crown, whose centre is situated some degrees lower, near the south east, at least in all places where this remarkable modification of the phenomenon has been observed. But if we determine the apparent position of this crown, either by the aid of astronomical instruments, or by observing what stars are comprehended within it at the time of its formation, we shall find that its centre, in every place where it has been observed, is always situated exactly in the

direction of that point in the heavens, to which the magnetic needle is directed, when suspended by its centre of gravity, in such a manner as to admit of its taking its position freely, in obedience to the resultant of the magnetic forces exerted upon it by the terrestrial globe. I have myself* had an opportunity of verifying most of the particulars here mentioned in the case of a very large aurora borealis, which was visible on the 27th of August, 1817, during my visit to the Shetland Islands.

We first saw in the northeastern parts of the horizon, several slender jets of light, which, having attained a little elevation, continued to shine for some time and then vanished; but in about an hour and a half afterwards they re-appeared in the same region of the heavens, and were now much stronger, more brilliant, and more extended. Very soon a regular arc resembling a rainbow began to present itself just above the horizon. It was at first incomplete, but gradually increased; and after some moments, I saw the other part approaching from the west, and upon being formed, it ascended instantaneously, accompanied by a multitude of jets of light which rushed towards it from all parts of the northern horizon; then the summit of the curvature rose almost to the zenith. This arc was at first wavering and unsettled, as if its component parts had not taken a stable position: but very soon the agitation entirely ceased, and it remained in undisturbed beauty for more than an hour, having only a progressive motion, and that almost insensible, towards the southeast, whither it seemed to be carried by a gentle north-western breeze that was then blowing. So that I had sufficient time to examine it, and to fix its limits and position with the circle used in my astronomical observations. I found that it comprehended a portion of the horizon amounting to 123° 42′, and that its centre was situated exactly in the direction of the magnetic needle. The whole region of the atmosphere embraced by this arc in the north-western part of the heavens, was incessantly traversed in all directions by luminous jets, whose different forms, motions, colours, and durations, engrossed my imagination no less than my senses. Most frequently, each jet at its first appearance was a mere stream of whitish light; its size and brightness rapidly increased, and it occasionally presented some very singular variations of direction and curvature. When completely developed, it contracted into a slender rectilineal thread, for the most part exceedingly

*T. B. Biot.

brilliant, and tinged with a very deep red colour. After this it grew fainter and fainter till it finally vanished, often at the very place where it first appeared. The long continuance of many jets in the same apparent place, considered in connection with the infinity of shades assumed by them, seems to prove, that the light is not reflected, but direct, and that it is actually developed in the place where it is first seen; besides, I have not been able to discover in it the least trace of those physical properties which characterize reflected light; and which are designated by the term *polarization*. All these fires and even the arch that comprehends them, occupy a region more elevated than the clouds; since the clouds themselves intercept them; and the edges of these clouds were actually or seemed to be tinged with light. The moon, which had then reached a considerable elevation above the horizon, shed her lustre also on this imposing scene, and the tranquility of her silver light formed a most agreeable contrast with those vivid corruscations with which the atmosphere was inundated.

Having now given a view of the principal circumstances attending this phenomenon, we propose to deduce from them the conditions of its existence; and the first thing to be determined is, whether it exists in our atmosphere or beyond it. There is a simple method of settling this question. If it be beyond the atmosphere, it must be independent of the diurnal rotation of the earth; and therefore its jets of fire, its arcs, its luminous crowns, must follow the general course of the stars from east to west, and seem like them to turn about the celestial poles. On the contrary, if the meteor belongs to our atmosphere it should partake of the common motion which the rotation of our globe communicates to all terrestrial bodies, even to the clouds; it should then appear to be immovable with respect to these bodies, or at least to undergo only accidental disturbances like the clouds themselves. All observations unite in establishing the latter supposition; and the length of time during which the meteor, observed by me at the Shetland Islands, continued, would, if necessary, afford a fresh confirmation.

We may then consider it as an established fact, that the phenomenon of the aurora borealis takes place in our atmosphere. But, as is well known, elevated objects when seen at a distance through the atmosphere, are apt to produce many optical illusions. For example, all the stars seem to us attached to the concave part of the same spherical surface or dome: although their distances are infinitely various. The

vast trains of luminous vapour which form the tails of the comets, seem also to apply themselves to this dome, although in reality they stretch into space in rectilineal directions. By another illusion, when the sun is partially concealed behind a mass of clouds, and emits rays of light through the openings of these clouds, the rays, although actually parallel, appear to converge towards the point of the heavens where the sun is. These general laws of perspective must affect, in like manner, the appearance of the luminous jets emitted by the meteor in question, and must be taken into consideration in our attempts to explain them. But from whatever situation these jets are observed, they always seem to describe arcs of great circles on the celestial dome, and to converge towards that part of the heavens to which the needle points when perfectly free. Whence we conclude, that they are in reality cylindrical, and parallel to the direction of the needle. But each jet presents, moreover, great varieties of size and lustre, from which we are led to believe that they are, in fact, composed of a great number of shorter cylinders independent of each other, and in part piled one above another. As these indications are noticed throughout the whole region of space where the meteor is visible, we may conclude, with geometrical rigour, that it consists of a forest of luminous columns, all parallel to the resultant of the magnetic forces, and of course for short distances parallel to each other, and suspended at nearly equal heights on different sides of the horizon. These columns being situated at different distances from the observer, must, by the perspective effect, appear to be raised to different heights. They must also mutually cover each other, and appear to project one over the other, especially when, being seen near the horizon, the visual rays proceeding from them are nearly perpendicular to their length; but after attaining such an elevation that their intermediate spaces may be seen, they must appear to separate; if then a certain number of them be simultaneously transported over the head of the observer, in such a manner as to pass by the point of the heavens to which the magnetic needle, parallel to them directs itself, the projection of all these columns on the celestial dome, will form about this point a luminous crown, the divergent rays of which will seem to descend on all sides toward the horizon, till they arrive at the apparent height at which the meteoric columns will have descended by the effect of the progressive motion.

This constitution of the meteor, which has been deduced

from optical considerations, is rendered probable by many curious facts, which different observers have had occasion to notice, and which have a relation to the positions which these different parts of the meteor happened accidentally to have with respect to them.

For example, when the meteoric colonnade, already illuminated, is situated entirely in the horizon exactly north of the observer, if it happens to be transported in a southerly direction, and in consequence to approach the observer, without any disappearance, or change of arrangement, of the columns composing it, we ought to expect the same optical effect which is presented by the trees of a forest when we approach them; that is, the columns situated eastward will separate toward the east, and the columns situated westward of the plane of the magnetic meridian, will appear to separate toward the west, while those which are in this meridian will appear to be stationary, or at least only to ascend directly towards the zenith. This appearance was attentively observed by F. C. Mayer, at Petersburg, in a large aurora borealis, which was seen on the 16th of Sept. 1726. I will quote his very language, observing that by the word "trabs," he designates a vertical jet, or one of our luminous columns. He first describes the formation of an arc, whose summit was not directed exactly to the north, but which had a very considerable declination to the west. He then adds, "Motus trabium mirus erat; quæ enim in occidentali arcûs parte extabant, versùs occidentum ferebantur; ad orientem ferebantur, quæ in orientali arcûs parte sitæ erant; boreales autem trabes stabant immobiles. Ex hoc phænomeno intellexi lucem moveri ex nord-west versùs verticem meum, id quod sequentibus phænomenis confirmatum est." It will be seen that Mayer has deduced precisely the consequences which are required by the rules of perspective.

Another case which may sometimes present itself, although very rarely, occurs when the illumination of the meteoric colonnade, seemingly accidental, appears for some time to take place only over a certain number of the columns which compose it. Then if these columns are placed at sufficient distances from each other, we may have an opportunity of examining them singly. This opportunity was afforded by the remarkable aurora borealis of 1716, an account of which may be found in the memoir of Dr Halley, (Phil. Trans. 347, p. 411, 415.) Small columns of equal lenghts and parallel to each other were distinctly seen separate in a portion of the

heavens surrounded by two luminious and almost horizontal belts. An account of a like phenomenon may also be found in another memoir of Dr Halley, (Phil. Trans. No. 363, p. 1099, for the year 1719.) He there relates, that from time to time, there appeared in the air at a great height collections of columns, or co-ordinate luminous beams, resembling the pipes of an organ, which presented themselves to view as suddenly as if a curtain had been drawn from before them. Indeed, if any one will undertake to read the numerous accounts of this meteor which have been furnished by those who have visited the northern regions, he will find a mass of facts which perfectly answer to the constitution of the meteor as deduced by us from the laws of perspective, and he will not meet with any thing opposed to our conclusions. A full statement of these geometrical deductions has been given by Dalton, probably without being aware that they had been obtained already by Cotes, in 1716, the person of whom Newton said, that "if he had lived, we should have known something;" and that they had since been adopted by Cavendish, the most severe and cautious of all philosophers. I have made this remark in order to show that they may be regarded as rigorous.

After having given a general description of the meteor, one of the most essential circumstances to be determined is its elevation. Attempts have been made without number to ascertain this point, by the aid of the same processes which geometry affords for measuring the distances of inaccessible objects; that is, by observing in different places, at the same time, the position of the same part of the meteor. But the difficulty of obtaining this perfect identity as to time and point of the object, renders the application of the method very uncertain; and accordingly the results obtained by it assign to the meteor uncertain heights, varying, in some cases, from twenty to more than one hundred leagues. Still more uncertainty prevails with respect to the length of the meteoric columns, which some have attempted to measure by like processes. If, in fine, the estimates made under certain favorable circumstances appear worthy of confidence, it may be urged, I think, that they are not general; and that in certain cases at least, the meteor descends much lower than we should thus be led to suppose. This seems probable from the quick and continual agitation of the phosphoric jets, the simultaneous progressive motions of the arcs, like that which a gentle breeze might be expected to give them, the

slow and regular transfer of those fleecy portions of phosphoric matter, which travellers in the northern regions assure us they have often seen floating separately in the atmosphere; and I myself saw a like phenomenon at the Shetland Islands, the 6th of September, 1817. It was a dense cloud which slowly ascended above the horizon from the north-east. Its sides were the centres of a phosphoric light which seemed at one time to remain behind till it was extinguished, at another to break forth and illuminate the edges of the cloud. I can give no better idea of this phosphorescent appearance, than by comparing it to the dark clouds of our theatres when illumined by lamps from behind. Yet for some moments I observed on its inferior surface a small spot where the light seemed to intervene between it and me. This cloud, having attained a height of about 45°, remained for some time stationary, and then gently moved to the west, still retaining its phosphorescence; some jets of light also, proceeding from the northern horizon, inclined towards the west, as if a wind in the higher regions of the atmosphere, coming from the south-east, was transporting the meteor to other countries. Similar phenomena presented themselves on the 14th of September. These observations, from which we may infer, that the aurora borealis belongs to the region of the higher clouds, seem to me to render probable an opinion generally prevalent in all northern countries, which is, that the aurora borealis, when very vivid, is accompanied with a considerable noise, and in some cases with one of great violence. I am well aware how little reliance is to be placed on common opinion under circumstances calculated to inspire terror, or when influenced by the frightful appearance of rapid and unexpected commotions; but the assertions thus made, like all others, possess a degree of credibility; and if it is unphilosophical to believe without proof, it is equally so to reject without examination. Let a person apply himself for thirty years to the study of what are called popular prejudices, and I doubt not his labours would be rewarded by many valuable discoveries. If any one will inquire without bias or prepossession into the reality of the sounds alleged to proceed from the aurora borealis, I am persuaded that he will not hesitate to adopt the common opinion, so striking is the coincidence of testimony on this subject. The distinguished natural philosopher Muschenbroek, who wrote about the middle of the last century, reports, that this fact is generally affirmed by sailors employed in the whale fishery on the coast of Green-

land. Gmelin, in his account of Siberia, expresses himself in still more decided language; after speaking of the great splendor of the aurora borealis, as presented in these countries, he adds; "However beautiful this spectacle may be, I think it will be impossible to contemplate it for the first time, without emotions of terror; so constantly is it accompanied, as I have been informed by several intelligent persons, with noises like those hissings and cracklings produced by very large fireworks. The hunters who go in search of the blue fox to the confines of the Frozen Ocean, are frequently surprised by the unexpected appearance of this meteor; their dogs are frightened by it to such a degree that they cannot be kept from stopping and lying on the earth until the noise has ceased." There is a phrase belonging to the language of this country, used solely to express the terror which this phenomenon occasions. Gmelin adds, that there was a unanimous voice in support of what is here stated. I can affirm, moreover, that among the inhabitants of the Shetland Islands, the testimony is no less full and complete, although they do not speak of so loud a noise; a difference to be attributed undoubtedly to the less northern situation of these islands. M. Edmonston, who, like myself, was unacquainted with the passage just quoted from the work of Gmelin, described to me the noise occasioned by the aurora borealis in very similar terms, giving me to understand that he had very frequently heard it himself; he thought it most like the noise proceeding from a large fire. I did not have an opportunity of observing it during the appearance of the meteor when I was at Unst, as the sea then roared with great violence on the side of the island where I was. In fine, the inhabitants spoke only of having heard the noise of the meteor, when the phosphoric jets are very numerous, and when they cross and intermingle with the greatest activity. For the truth of what is here alleged, we may appeal with confidence to the whole population of the Shetland Islands; hardly a person is to be found who will deny having heard this noise; we do not however depend on assertion merely; it is described in the same manner by different persons, without their once imagining that there can be any doubt about it. The phenomenon seems to be much more brilliant a few degrees nearer the pole. M. Edmonston, in an account of the appearance of a large aurora borealis which he observed at Unst on the 1st of November, 1818, has afforded me a striking example of this difference. "I am now in compa-

ny," says he, "with two credible persons, who on a voyage from London to the Shetland Islands, were driven by winds to the latitude of $63\frac{1}{2}^{\circ}$, near the northernmost point of the islands. While they were in this latitude an aurora borealis appeared; the noise with which it was accompanied was such that the sailors were afraid to remain on deck; and it sent forth so strong a light, that we were able to observe the compass by it." It seems probable after this mass of testimony, that the meteor sometimes descends so low as to allow us to hear the noise proceeding from it. It has even been affirmed by Bergmann, that persons travelling over the Norwegian Alps have been enveloped in it, and have perceived a strong smell of sulphur, supposed to come from it.

Having thus collected the several particulars belonging to the aurora borealis, in doing which, I have endeavoured to exclude every thing of a hypothetical nature, we may consider this meteor as consisting of real clouds, proceeding usually from the north, and composed of some very light substances or at least of some substance so finely pulverized as to be capable of floating a long time in the atmosphere, endued with the property of occasionally becoming luminous; and especially (which is very important) sensible to terrestrial magnetism, and spontaneously arranging themselves in columns which turn towards the earth, as real magnetic needles would do. But of all terrestrial substances only the metals, so far as we know, are in any considerable degree susceptible of magnetism. It is then probable, that the columns of the meteor are at least in a great measure composed of metallic particles reduced to powder of extreme fineness. But this conclusion leads also to another; we know that all known metals are excellent conductors of electricity. Now the different strata of which the atmosphere is composed are usually charged with very unequal quantities of electricity; for if, when the atmosphere is most serene, we raise a paper kite with a metallic string, we may observe at the end of the string signs of electricity, ordinarily of the vitreous kind; and if, on the other hand, having ascended in a balloon, we let fall below the car a wire whose inferior extremity shall reach the lower strata of the atmosphere, we shall find, as has been observed by M. Gay-Lussac and myself, that the superior end of the wire gives indications of resinous electricity. Accordingly, if columns consisting in part of metallic substances, are suspended in nearly a vertical position in the atmosphere, like the columns of the aurora borealis when they float over

regions adjacent to the pole, the electricity of the atmospheric strata at the summit and base of the columns will find in them so many conductors more or less perfect; and if this tendency of electricity to diffuse itself uniformly is sufficient to overcome the resistance arising from the imperfect conducting power of the columns, it will flow along these columns, illuminating its path, as is often observed in conductors which are not continuous. When this passage takes place in the higher regions of the atmosphere where the air, on account of its rarity, offers very little resistance, the electricity will *flow on silently with all those variations of light which we* observe in exhausted tubes. But if it extends itself to the inferior strata, it must necessarily occasion such hissing and crackling noises, as are found to accompany the aurora borealis, when it descends near the surface of the earth. In fine, as the meteor is visible only by means of this accidental circumstance, there is reason to believe that it may exist in the air and exert an influence over the magnetic needle without being perceived; it is also very possible that it may be bright in some places and obscure in others; while under certain circumstances the disturbance of the electric equilibrium being sudden and general, the whole meteoric colonnade may be instantaneously illuminated. These phenomena must be less striking as the meteor advances over the more southern countries, not only because it has then extended itself more widely, but especially because the conducting columns, always conforming to the direction of the magnetic needle, will become more and more horizontal, and will have their two extremities in atmospheric strata less distant, and therefore less unequal with respect to the quantities of electricity with which they are charged; a greater humidity also which prevails in the lower latitudes is favourable to a frequent discharge.

All these results, agreeing so exactly with what we have collected from actual observation, it will be seen, depend solely on the idea, that the columns of which the aurora borealis is constituted, are partly, at least of a metallic nature. This agreement with known phenomena considerably increases the probability of the supposition to which we were previously led by the magnetism of the meteoric columns; the mutual connection and intimate dependance, thus easily established between phenomena so numerous, and, at first view, so remote, give an air of reality to the whole, seldom to be met with in physical theories which have not the basis of established fact.

But, independently of the luminous jets which may thus be produced by the simple passage of electricity along the metallic columns, a passage which in virtue of a property lately discovered, might of itself be sufficient to magnetize these columns; we can hardly help considering the phenomena in question as proceeding from an actual combustion in the phosphoric clouds, which, detaching themselves in some cases from the burning meteor, as affirmed by many observers, and as I have myself seen, transport with them the principle of their phosphorescence, and emit at intervals jets of *light resembling rockets, which leave after them a whitish train.* We must then regard it as at least a very probable supposition, that the aurora borealis is composed of substances, capable, occasionally of inflammation, either of a spontaneous kind, or in consequence of a discharge of electricity from the clouds which contain it; a very powerful mode of combination, of which we have frequent instances in our laboratories.

Such are the physical conditions on which the aurora borealis seems to depend, and which are deduced directly from the phenomena presented to us. Whence then is derived the matter which constitutes it? To this question we can as yet give only a doubtful answer; but if a skilful observer would, for several winters, carefully study every circumstance belonging to these phenomena as they present themselves in the northern regions, with all the helps that the sciences can furnish, something decisive would probably be learned.

ART. XV.—*On the Mirage.* Communicated for the Journal.

[A few years ago, as the inhabitants of a small village on the coast, not many miles east of the Piscataqua river, were issuing from the village church on Sunday, after services, on a fine summer morning, they were attracted and alarmed by the vision of a ship, with her sails set and filled, moving magnificently in the air above the ocean, and directing her course westward, towards the land. For a few moments they continued in silent wonder; but a light breeze from the land sprang up, the glorious object vanished in the soft air, and no trace was left of its appearance, but the superstitious awe which remained in the minds of the spectators. This is not a solitary instance. Similar appearances have been at other times observed or related, but have been usually ascribed by the sceptical part of the world, to some foolish imagination or some idle fear that possessed and deceived the minds of the vulgar.

Again, stories have been told of men with a power of vision so penetrating, that they could descry a vessel, and predict her approach when she was many leagues at sea, beyond the reach of the eyes and glasses of all around them. Witchcraft or previous intelligence or conjecture have been brought in to account for the fact, or it has been disbelieved and rejected.

It is easy to say that we do not believe; it is more philosophical often to confess that we do not understand, and to suspend our judgement until we have examined all the circumstances; and we may sometimes find that researches in the kingdom of nature bring to light principles and facts which give a probable foundation for the common belief of the multitude; as the progress of discovery in modern times has shown to be true, what were long received only as the old man's stories of the garrulous father of history. This we shall find to be true with regard to the exaggerated accounts of the appearance spoken of above. Stripped of its popular dress, we shall find it to take its place among a thousand similar facts perfectly well established. Of the most remarkable of these facts and appearances, it is proposed to give some account. We are led to do this at the present time, not from having any new facts or views to communicate, but from having had our attention drawn to it by lately observing the phenomenon, and finding from conversation that very few of those otherwise conversant with science seemed well to understand the cause or to have read the best accounts that have been given of it. It is known among sailors by the English word *looming*, or by the French word *mirage*, under which name it has been spoken of by men of science.]

The first well described observations on the mirage, or extraordinary refraction, were made by Mr Joseph Huddart, at Allonby, in Cumberland, in 1793.* He observed, at different times, a point of land and a vessel, seen at the distance of about seven leagues over the water. Inverted images of objects were seen immediately under the real objects. He supposes these images to be produced by rays passing downwards from the objects, and being refracted upward again, in a path convex to the earth, by passing through a medium of air, of which, in consequence of evaporation, the greatest density was not at the surface, but a few feet above the surface of the water. This medium he thinks would give an upward curvature to the rays, by which means this would come to the eye as from some point below the real object, and as if reflected from a surface a few feet above the true surface of the water.

In this way he accounts for the blunt appearance of the really sharp points of a promontory seen at a distance; the object seen by the eye being made up of a true point and its reflected image. He supposes, with apparent reason, that this principle will sometimes cause lights at a distance to

* See Philosophical Transactions for 1797, p. 29.

seem brighter than when seen nearer, as the real and reflected or refracted image, will, in that condition, become nearly coincident.

In the summer of 1798, the French army under Bonaparte passed through the desert of Lower Egypt, where they daily observed the phenomenon of which the following description is given by Monge.*

"The country of Lower Egypt is nearly a level plain, which loses itself, like the sea, in the clouds at the extremity of the horizon; its uniformity is only interrupted by a few eminences, either natural or factitious, on which are situated the villages, thus kept out of the reach of the inundations of the Nile.

"Both morning and evening the aspect of the country is exactly as it ought to be; and between you and the last villages which present themselves, you perceive nothing but land; but when the surface of the earth is sufficiently heated by the rays of the sun, and indeed until it begins to get cold towards the evening, the land no longer seems to have the same extension, but to be terminated to within the distance of about a league, by a general inundation.

"The villages placed beyond that, appear like so many islands stationed in the midst of a great lake, from which the spectator is separated by an extent of land, more or less considerable according to circumstances. You then behold the image of each of these villages reflected exactly as if it were exhibited on a clear surface of water, with only this difference, that as the representation is at a considerable distance, the smaller objects are invisible, and the masses alone distinct; in addition to this, the edges of the reverted image are ill defined, and such as they would be if the surface of the reflecting medium happened to be a little agitated.

"In proportion as one approaches a village, which appears to be placed in the midst of an inundation, the margin of the water seems to recede, and the arm of the sea, separating you as it were from the village, shrinks back by degrees; it at length disappears entirely, and the phenomenon which now ceases, in respect to the first village, is instantly reproduced by a new one, which you discover at a due distance in the rear. Thus every thing contributes to complete an illusion, which is sometimes cruel, more especially in the desert, because it tantalizes you with the appearance of water, at a time when you experience the greatest want of that element."

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*Memoirs relative to Egypt, p. 74.

This is explained by Monge by supposing that the air in contact with the earth is heated to a very great degree by the direct rays of the sun, and so continually heated, that, although a portion of it is constantly rising, in consequence of its greater levity, still the portion nearest the earth is, taken altogether, a more rare medium than that at a greater distance. Now when light passes from one medium into another, or from a medium of one degree of density into a medium of another, a portion of the light is reflected at the common surface of the two media. The quantity of light reflected is in proportion to the magnitude of the angle of incidence. The light coming from the lower regions of the heavens strikes this surface of lighter air at a great angle, and consequently the image of the lower part of the sky is reflected to the eye of the observer. But this reflection can take place only at a considerable distance from the eye of the observer, as it is only at such a distance that the angle of reflection or its equal, the angle of incidence, is sufficiently large. All the rays which reach the eye from any point on the surface nearer than a league's distance, come under an angle less than the lowest limit of reflection.

If the same phenomenon should take place in a situation where there were no elevated objects to show the true limits of the horizon, the sky would seem to be prolonged, the apparent horizon would be depressed, and would seem much nearer than it really is. But where villages or trees occur beyond the nearest limit of reflection, they point out the real situation of the horizon, and the image of the heavens which is seen within that horizon, will at once assume the appearance of a surface of water. Terrestrial objects occurring in such a situation will seem to be reflected from the image of the sky between the observer and the objects, in the same manner as they would be from a surface of water, with this difference, that the reflected image will be less distinct, and will have the appearance of vibrating, from the undulating motion of the surface of air from which they are reflected.

The next observations are the extremely curious but inaccurate ones, of which the following account is abridged from a very interesting paper in the Philosophical Transactions for 1799, p. 13. In this paper, Prof. Vince details some observations made at Ramsgate, in a sultry evening, after an extremely hot day, in August, 1798, under a clear sky, from elevations sometimes of 25, at other times of 80 feet above the water, with a telescope magnifying 30 or 40 times.

He first directed the telescope towards a ship, the topmasts of which were just visible above the horizon. At the same time he discovered in the field of view, two complete images of the ship in the air, directly over the ship itself, the lower image being inverted, and the upper one erect, having their hulks joined. As the ship was receding from the shore, less and less of its masts became visible, and, as the ship descended, the images ascended, but as it did not sink entirely below the horizon, he had not an opportunity of observing in what order and at what time the images would in that case have vanished.

On directing his telescope towards another ship, whose hulk was visible in the horizon, he observed a complete inverted image, the mainmast of which just touched that of the ship itself, and which moved as the ship moved, without any variation in its appearance. In this case there was no second image.

The next ship to which he directed his telescope was so far on the other side of the horizon, as just to prevent its hulk from being seen. At first, only an image of the topsail and of the mast, joining that of the ship, and an image of the bowsprit were visible. These images would suddenly appear and disappear very quickly after each other, first appearing below and "running up very rapidly, showing more or less of the masts at different times, as they broke out; resembling, in the quickness of their breaking out, the shooting out of a beam of the aurora borealis." The ship was receding, and he found that, as it descended beyond the horizon, more of the image gradually appeared; till an image of the whole ship was completed, with the mainmasts touching each other. As the ship descended lower, the image and ship separated, but no second image was observed.

On moving the telescope along the horizon, he observed the top of the mast of a ship, just visible. Directly over this, at some little distance, was an inverted image of a vessel, and above that, an erect image, both of them very perfect and well defined, and an image of the sea between them, the water appearing very distinctly. As the ship approached the horizon, the upper, erect image gradually disappeared, and vanished, and after that the image of the water disappeared, and the lower, inverted image descended. He thinks that if he had directed his telescope to that point sooner, he should have seen the two images before any part of the ship itself was visible.

Upon turning the telescope towards the cliffs at Calais, which were visible, he observed an image of the cliffs above the cliffs themselves, with an image of the sea between them. Afterwards an inverted image of a ship appeared against the cliffs, while the hulk of the ship was below the horizon. He supposes from the appearance of the fog soon after in that direction, that there was, at the time he made these observations, a considerable fog on the other side of the horizon.

These observations were often repeated, and the images in general well defined. The usual refraction at the same time was so great, that the cliffs at Calais were visible at the water's edge, whereas they are frequently not to be seen in clear weather, from the high lands about the place.

To *explain* these appearances, Prof. Vince supposes three rays to reach the eye from the object. One ray is subject to the ordinary refraction, and exhibits an image in the natural way, a little above the true place of the object. A second ray is supposed to pass above the first ray through air "varying quicker in its retractive power" than the air through which the first ray passed, and therefore having a greater curvature, the convexity being upwards and presenting a second image; "in like manner, if a quicker variation of refractive power should take place above this curve," a third ray may describe a third curve still higher, also convex upwards, and presenting a third image still higher than either of the former. "The alterations of refractive power," Prof. Vince supposes, "may arise partly from the variations of its density and partly from the variations of its moisture, and the passages of the rays through the boundary of the fog, may there suffer a very considerable refraction."

In Scoresby's voyage to Greenland, in the summer of 1822, he observed many curious phenomena of this kind, which he has extremely well described. The first description we shall extract is the following:

"In the evening it was calm, with a brilliant sun, and very curious exhibitions of the phenomena of unequal refraction. Hummocks of ice assumed the form of castles, obelisks, and spires; and the land presented extraordinary features. In some places, the distant ice was so extremely irregular, and appeared so full of pinnacles, that it resembled a forest of naked trees; in others it had the character of an extensive city, crowded with churches, castles, and public edifices. The land was equally under the influence of this singular mirage. Huge masses of rocks and summits of mountains were reared

to an enormous elevation, distorted into singular shapes, and often seemed to be detached from the rest of the land, and freely suspended in the air. The horizon, bounded by ice, which ought to have been pretty regular and uniform, was sometimes undulated and broken."

Another scene observed within a few days of the same time, may be found described in page 217, Vol. I, of this Journal.

He describes the following appearances as having been observed on July 9th.

"The morning was brilliant, with light winds, and a highly refractive state of the atmosphere. The land, and a number of ships that came within sight, as well as the ice, and indeed all distant objects, were strangely distorted. Inverted images of two ships, occasionally double, were seen in the air, which, I imagine, were at least ten miles beyond the limit of direct vision; for we approached them about this distance without being able to see them. In addition to the phenomena observed and described on the 19th of June &c. I noticed several vessels that had their hulls elevated to the apparent magnitude of a castle; the height of the hull, in some instances, being equal to that of the masts; in two or three positions, the courses seemed to be separated twenty or thirty yards from the hull, instead of being nearly in contact. So unequal was the refraction, and so various in its effects, that while in one ship the masts were uniformly expanded, or the hull magnified,—in another ship the courses and topgallant-sails were heightened, and the intermediate sails, the top-sails, contracted to one fourth their proper size,—and in a third, a very little distance from this, the courses and topgallant-sails were contracted, and the top-sails expanded. In all these examples, the peculiarities were continually varying. No sooner had one appearance been examined and sketched, than it changed, and often exhibited the most uncouth proportions. The distant ice partook also of the same influence, and presented very extraordinary and often beautiful architectural structures. These interesting exhibitions were at length closed by the commencement of a dense fog." p. 143.

Again,

"As soon as the fog had cleared away, on the 16th, the influence of the sun that was then most powerful, soon began to produce a rapid evaporation from the surface of the sea and ice,—this gave rise to the optical phenomena of unequal refraction, which continued to operate with ever varying effect,

for sixty or seventy hours. At one period (about 10 P. M. of the 16th) the phenomenon was so universal, that the space in which the ship navigated seemed to be one vast circular area, bounded by a mural precipice, of great elevation, of basaltic ice. In some places, where water, instead of ice, was seen through the refracting medium, the cliff it seemed to constitute had the colour and appearance of real basaltic columns; and where both water and ice were refracted, the specks and patches of ice had sometimes so much of the character and appearance of land, that one of my principal officers, who was familiar with the general phenomena, was deceived by it."

On the 18th, he says,

"This evening, about 9 P. M., the refracted ships, with their repeated images, were most beautifully and distinctly defined, and especially the latter, which appeared as usual, in an inverted position in the air. Of some vessels, whose hulls were beyond the horizon, there were two, and of one ship three, distinct inverted images, each exhibited in a different stratum of refracted ice, one above another,—the lowest image being at an altitude of more than the apparent height of the ship's mast, above the mast-head of the original. And of two vessels there were well defined images, in an inverted position, though the ships to which they referred were not within sight! It should be observed, that the inverted images were visible on this occasion, only, when an appearance of ice, produced either by reflection or refraction, occurred above the regular line of the horizon, in the quarter occupied by the ships: in the clear intervals of the lower atmosphere between the strata of refracted ice no image was seen; and when the stratum was too narrow to comprise the whole of the image, a part of it only appeared. And it should be also observed, that these phenomena were principally telescopic, both the ships and the images being so distant, that, to the naked eye, they only appeared as indistinct specks."

The appearance of the land was no less remarkable than that of objects at sea.

"In many places there were patches resembling two obtuse pyramids united by their apices, the upper one, though quite as distinct as the other, being evidently the inverted image only of the lower one. In occasional positions, where two of these double pyramids were near together, the upper limbs of the higher pyramids coalesced, so as to present the appearance of prodigious bridges, some leagues in extent, with a clear atmosphere beneath them. Sometimes these pyra-

mids were so compressed and multiplied, that three or four were seen in a vertical series, forming so many distinct horizontal strata, joining in the middle, but all detached at the extremities.

The general telescopic appearance of these coasts, when under the influence of unequal refraction, is frequently that of an extensive ancient city, abounding with the ruins of castles, obelisks, churches, and monuments, with other large and conspicuous buildings. Some of the hills often appear to be surmounted with turrets, battlements, spires, and pinnacles; while others, subjected to another kind of refraction, exhibit large masses of rock, apparently suspended in the air, at a considerable elevation above the actual termination of the mountains to which they refer. The whole exhibition is frequently a grand and interesting phantasmagoria. Scarcely is the appearance of any object fully examined and determined, before it changes into something else. It is, perhaps, alternately a castle, a cathedral, or an obelisk; then expanding and coalescing with the adjoining mountains, it unites the intermediate valleys, though they may be miles in width, by a bridge of a single arch of the most magnificent appearance."

"The cause of these phenomena, as far as they depend on refraction, is," Scoreby supposes, "the rapid evaporation which takes place in a hot sun from the surface of the sea; and the unequal density occasioned by partial condensations, when the moist air becomes chilled by passing over considerable surfaces of ice." This is sometimes visible to the eye and gives a tremulous motion to all bodies on or near the water. It does not, however, he supposes, extend to a great distance above the surface, since objects as elevated as the top of the mast, from which most of his observations were made, were often not affected by it, while every object near the surface was very much so. The medium of the atmosphere, near the water, he supposes to be made up of parallel strata of different densities, often very unequal and changeable. Objects seen through such a medium must be unequally elevated and distorted, while objects seen through a medium of a uniform density, or of a density uniformly increasing or diminishing, would be regularly elevated to a less or greater degree, in proportion to the density of the medium and the distance of the object.

In the following paragraph, Scoresby gives the reason why this curious phenomenon has been so seldom noticed or described by navigators.

"As the more extraordinary and beautiful effects of unequal refraction cannot be fully discovered, without the use of a telescope, they escape general observation; and as the looming of the distant ice, or distortion of objects, is the most common effect of this state of the atmosphere, it becomes a considerable annoyance to persons not interested in the phenomena. As in this case, the vapour gives an indefinite and tremulous outline to every object beyond a certain distance; —ships, a mile or two off, cannot be recognised;—a wall of ice seems to surround the navigator, the openings and leads in which cannot be discerned at a distance;—and, of the actions and employment of remote vessels, within sight, a knowledge of which is often of great importance to the unoccupied fisher, no correct conception can be formed.

Some of the appearances described by Scoresby have a strong resemblance to the phenomenon which is seen at times in the Straits of Messina, and which has been long known and often described under the name of the Fata Morgana.

One of the earliest descriptions of it, is that of father Angelucci, as quoted in Swinburne's Travels.

"On the 15th of August, 1643, as I stood at my window, I was surprised with a most wonderful, delectable vision. The sea that washes the Sicilian shore swelled up, and became, for ten miles in length, like a chain of dark mountains; while the waters near our Calabrian coast grew quite smooth, and appeared as one clear polished mirror, reclining against the aforesaid ridge. On this glass was depicted, in *chiaro scuro*, a string of several thousands of pilasters, all equal in altitude, distance, and degree of light and shade. In a moment they lost half their height, and bent into arcades, like Roman aqueducts. A long cornice was then formed on the top, and above it rose castles innumerable, all perfectly alike. These soon split into towers, which were shortly after lost in colonnades, then windows, and at last ended in pines, cypresses, and other trees, even and similar This was the Fata Morgana, which, for twenty-six years I had thought a mere fable."*

A very full and philosophical account is given of it by Minasi.

"When the rising sun shines from that point whence its incident ray forms an angle of about forty-five degrees on the sea of Reggio, and the bright surface of the water in the bay is not disturbed either by the wind or the current, the spec-

* Polehampton's Gallery, vol. p. 513.

tator being placed on an eminence of the city, with his back to the sun and his face to the sea;—on a sudden there appear in the water, as in a catroptric theatre, various multiplied objects; that is to say, numberless series of pilastres, arches, castles well delineated, regular columns, lofty towers, superb palaces, with balconies and windows, extended alleys of trees, delightful plains with herds and flocks, armies of men on foot and horseback, and many other strange images, in their natural colours and proper actions, passing rapidly in succession along the surface of the sea, during the whole of the short period of time while the abovementioned causes remain.

"But if, in addition to the circumstances before described, the atmosphere be highly impregnated with vapour and dense exhalations, not previously dispersed by the action of the wind or waves, or rarefied by the sun, it then happens that in this vapour, as in a curtain extended along the channel to the height of about thirty palms, and nearly down to the sea, the observer will behold the scene of the same objects not only reflected from the surface of the sea, but likewise in the air, though not so distinct or well-defined as the former objects from the sea.

"Lastly, if the air be slightly hazy and opake, and at the same time dewy and adapted to form the iris, then the abovementioned objects will appear only at the surface of the sea, as in the first case, but all vividly coloured or fringed with red, green, blue, and other prismatic colours."*

Minasi supposes these different appearances to be produced by the reflection of objects on the neighbouring coasts from the surface of the water in the strait, by refraction produced by saline and other effluvia, that is, by evaporation and by fogs.

(To be Continued.)

ART. XIV.—*Notice of the Periodical Comet of* 1819.† By M. Damoiseau. (Translated for this Journal from the *Connaissance des Tems*, for 1822. By Seth Ames, A. B.)

SOON after the discovery of this comet by M. Pons, at Marseilles, it was ascertained that its movement was periodical,

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* Polehampton's Gallery, p. 511.

† This notice is an extract from a memoir presented by M. Damoiseau to the Academy of Sciences, in the month of February last, and is the subject of M. Poisson's report, which may be found, p. 224 *Connais. des Tems.*

it had already been observed in 1786, 1795, and 1805, and was afterwards observed at 1822, at Paramatta. In order to be able to predict its next return to its perihelion, it is indispensably necessary to ascertain the irregularities produced in its motion by the disturbing force of the planets. The observations of 1786 were not sufficient to ascertain its orbit, and those of 1795, exhibit but little precision; but the elliptical orbits of M. Encke, which represent so exactly the observations taken in 1805, 1819, and 1822, have served as the basis of the actual research. I present, in a number of tables, the results of a calculation, relating to the alterations of the comet's orbit in these two last periods, and the two following, with their application to its future state in 1825 and 1828. The method and the details of these different calculations may be found in a memoir presented to the Academy of Sciences.

Jupiter, the Earth, and Venus are the only planets whose action has been considered; the following are the results in relation to the mean anomaly and mean dialy motion.

Periods.	Intervals observed.	Alteration of the Mean Anomaly.		Alteration of mean daily Motion.
1805 to 1819	4814',746	By the action of ♃	+15859''38	+ 3''5889
		♀ in 1809	— 476,76	— 0,1313
		in 1818	— 4,97	— 0,1105
		♁ in 1806	+ 64,01	+ 0,0178
		in 1818	— 5,14	— 0,0735
			+ 15436,52	+ 3,2914
1819 to 1822	1212',742	By the action of ♃	— 9939''38	— 7''4349
		♀ in 1819	+ 81,26	+ 0,0716
			— 9858,12	— 7,3633
1822 to 1825	- - - - -	By the action of ♃	+ 363''51	+ 0''7202
		♁ in 1822	— 56,90	— 0,0494
			+ 306,61	+ 0,6708
1825 to 1829	- - - -	By the action of ♃	— 705''25	— 04''420
		♁ in 1828	— 6,90	— 0,0986
			— 712,15	— 0,5406

If we designate the mean daily motions of the comet at its perihelion that of

1805 by n
1819 n'
1822 n''
1825 n'''
1829 n''''

and the intervals between passing the perihelion, from

1805 to 1819 by T
1819 1822 T'
1822 1825 T''
1825 1829 T'''

we shall have by the period from 1805 to 1819.

$$n = \frac{560^\circ \times 4 - 15436'',52}{T} = 1076'',6925 - 3'',2061$$
$$= 1073'',4864,$$
$$n' = 1073'',4864 + 3'',2914 = 1076'',7778,$$
$$n'' = 1073'',4864 + 3'',2914 - 7'',3633 = 1069'',4145;$$

by the period from 1819 to 1822.

$$n' = \frac{360^\circ + 9858'',12}{T'} = 1068'',6525 + 8'',1288$$
$$= 1076'',7813,$$
$$n'' = 1076'',7813 - 7''3633 = 1069'',4180,$$
$$n = 1076'',7813 - 3''2914 = 1073'',4899;$$

uniting the two periods, we have for the alteration of the mean anomaly in the interval between the perihelia of 1805 and 1822.

$$15436'',52 + 3'',2914 \times T' - 9858'',12 = 9570'',02$$

which gives

$$n = \frac{360^\circ \times 5 - 9570'',02}{T + T'} = 1075'',0745 - 1'',5877$$
$$= 1073'',4868,$$
$$n'' = 1073'',4868 + 3'',2914 - 7'',3633 = 1069'',4149,$$
$$n' = 1073'',4868 + 3'',2914 = 1076'',7782.$$

the values which are found have been formed into a table.

Periods.	n	n'	n''
1805 to 1819	1073″,4864	1076″,7778	1069″.4145
1819 to 1822	1073″,4899	1076″,7813	1069″,4180
1805 to 1822	1073″,4868	1076″,7782	1069″,4149

The values of the mean daily motion for each perihelion concluded from three periods, differ from each other by 0″,0035, 0″,0031, 0″,0004; on comparing them, two by two,

we find in this motion an augmentation in each revolution of 0″,00063, answering to a diminution of 0,00106 in the time of revolution. But the difference is above being of the order of quantities that are neglected in calculating the effect of disturbing forces, this retardation, if it exist at all, would not be perceptible till after a great number of revolutions.

It is now intended from the passages observed in 1805 and 1819 to ascertain that of 1822. According to the preceding observations, we have in relation to that period,

$$n' = 1076'',7778\text{ ; therefore,}$$

$$T' = \frac{360° + 9858'',12}{n'} = 1203',591 + 9',155 = 1212',746\text{ ;}$$

the observations give 1212′,742. We can hardly expect greater precision in the result.

An important object is to determine the instant of the next return of the comet to its perihelion. In this new revolution, as well as in that which follows, the effect of the disturbing force is very inconsiderable, and the elements undergo very little variation. We may hope then to assign with certainty, those parts of the heavens which the comet will occupy on its reappearance.

Take for n'' the mean value 1069″,4158 which results from comparing the three observed passages, we have

$$T'' = \frac{360° - 306'',61}{n''} = 1211',8766 - 0',2867$$

$$= 1211',5899\text{ ;}$$

this interval from May 24,4944, 1822,* the epoch of the passage to the perihelion, answers to Sept. 17, 0843, 1825, which will be the instant of its next return to that point.

As it may happen that from the configuration of the sun, the earth, and the comet about the time of this passage, the latter may perhaps be invisible, we will continue to apply the effects of disturbing forces in order to determine the instant of its second return to its perihelion in1829.

The mean value of n'' being 1069″,4158, we may conclude

$$n''' = 1069'',4158 + 0'',6708 = 1070'',0866\text{ ;}$$

and consequently,

$$T''' = \frac{360° + 712'',15}{n'''} = 1211',1170 + 0',6655$$

$$= 1211',7825\text{ ;}$$

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* The periods of absolute time in this notice are in mean time, reckoning from midnight meridian of Paris.

is the passage to the perihelion will take the 10, 8668, Jan. 1829, and we shall have

$$n'''' = 1070'',0866 - 0''5406 = 1069'',5460.$$

As to elements of the orbit, the following table shews the changes made in them in different periods.

Periods.	Alteration of Eccentricity.	Alteration of the longitude of the Perihelion.	Alteration of longitude of the Node.	Alter. of inclination of the Orbit.
1805 to 1819	+0,0019950	+5′, 1″,0	— 1′51″1	+ 2′ 48″3
1819 1822	—0,0039038	+9, 41,0	—10,50 0	—16 8 0
1822 1825	+0,0004305	+0, 15,2	— 0,10 1	+ 1 4 4
1825 1829	—0,0002920	+1, 19,1	— 0,39 2	— 0 54 6

By adding to the alterations in longitude of the perihelion and node 11′0″,4 for the first period, and 0′46″,3 for the others we shall have these alterations in relation to the moveable equinox.

By means of the preceding results, with the elements of 1822, corrected after the observations at Paramatta, the following table, for the times of passing to five perihelia has been formed.

Passage to Perihelion.	Eccentrick.	Longitude of Perihelion.	Longitude of Node.	Inclination of Orbit.	Mean daily Motion.	Semi major axis.
1805 Nov. 22,006	0,8464567	156°43′0″	334°18′29″	13°35′44	1073″4877	2,218912
1819 Jan. 27,752	0,8484517	156 59 1	334 27 36	13 38 33	1076 7791	2 214388
1822 May 24,494	0,8445479	157 11 29	334 19 32	13 22 25	1069 4158	2,224542
1825 Sept. 17,084	0,8449784	157 14 30	334 22 8	13 23 29	1070 0866	2,223611
1829 Jan. 10.867	0,8446862	157 18 35	334 24 15	13 22 34	1069 5460	2,224360

These are ephemerides to facilitate the search for the comet upon its returns. In 1825, the elongation of the comet from the sun, in the interval which the ephemeris includes, varies from 49 to 33 degrees, and accordingly we can hardly expect that it will be visible to us. And after it has passed to its perihelion, the circumstances are still less favourable. As to its return in 1828, the comet will be visible throughout Europe.

		Right ascension.	North'rn declination.	Distance of the Comet from the Earth.	Distance of the Comet from the Sun.	Intensity of Light.
1825.	July 14,075	59°6′	28°47	1,775	1,345	0,175
	24,073	70 5	30 52	1,597	1,198	0,273
	Aug. 3,086	84 15	23 56	1,439	1,041	0,445
	13,087	99 48	23 44	1,316	0,874	0,597
	23,061	118 5	21 28	1,243	0,697	0,647
1828.	Aug. 28,159	26°35′	23 17	1,437	2,175	0,096
	Sept. 7,175	25 47	24 58	1,276	2,073	0,143
	17,161	23 30	26 29	1,089	1,967	0,218
	27,183	19 24	27 50	0,920	1,855	0,344
	Oct. 7,191	12 57	28 43	0,774	1,737	0,552
	17,162	3 52	28 36	0,658	1,614	0,886
	27,157	352 33	26 51	0,574	1,483	1,380
	Nov. 6,173	340 23	23 13	0,521	1,345	2,033
	16,171	329 1	18 13	0,494	1,198	2,851

Report made to the Academy of Sciences. By MM. Laplace, Legendre, and Poisson, upon a Memoir of M. Damoiseau relative to the Disturbances in the Motion of the Comet of 1819, *for a short period.*

Till lately there had been but one comet whose return to its perihelion has been observed, and whose periodical time is known. This is the comet of 1682, which occupies about 76 years in performing its whole revolution. Halley discovered it to be the same with one which had already been observed, at two other epochs, in 1531, and 1607, and he predicted its return in 1758 or 1759. At this latter epoch, and before the comet had become visible, Clairaut calculated the instant of its passing its perihelion, having regard to the disturbances in its elliptical motion. The result of that immense labor differs only a few days, as is well known, from the time of the observed passage, which took place on the 12th of March, 1759. This remarkable phenomenon, of the reappearance of a comet at its perihelion, after one or more revolutions, has also occurred in our time with respect to the comet discovered in 1818, by M. Pons, who at that time observed it at Marseilles. The comparison of its parabolic elements with those of a comet observed in 1805 leads to the suspicion that the two bodies are the same. The remark was made at the Bureau des Longitudes, by M. Arago, when the discovery of the new comet was there announced;

it was also made in Germany by M. Olbers, who thought moreover, that this comet must be the same with that which had been observed in 1795 and 1786. According to this conjecture, its time of revolution can be only a very short period, and it is indispensably necessary to have regard to the ellipticity of its orbit in the calculation of its elements; this is done by M. Encke, astronomer at Gotha. The elliptic elements which he determined, either from the observations of 1805 or those of 1818 differ from each other still less than the parabolic elements, and there is no longer any doubt that they belong to the same body, the periodic time being nearly three years and a half. It has performed ed since its first appearance in 1805, four entire revolutions, and returned to its perihelion at the commencement of 1819. If we regard only the shortness of its successive revolutions this body might be considered a planet; but it has continued to be classed among comets, on account of the appearance it presents, and its not being visible to us in all parts of its orbit. To facilitate the observation of its return in 1822, M. Encke proposed to calculate an ephemeris relative to that epoch, but as the comet was for a great part of this new revolution at a very small distance from Jupiter, he was obliged to calculate the perturbations produced by the disturbing force of that planet, and he found in fact that in consequence of this principal action, the time of anomalistic revolution, the mean duration of which had been about 1204 days, from 1805 to 1819 would be augmented by about nine days in the following revolution. He announced that in 1822 the comet, in consequence of its declination, would not be visible in Europe, and that, in order to observe it, it would be necessary to go into the southern hemisphere. The event has completely verified this prediction. The comet returned to its perihelion in the latter part of May, 1822. It was observed at Paramatta in New Holland, by M. Dunlop, who first recognised it, and by Gen. Brisbane, and by M. Rumker, who determined its positions. The elliptic elements, deduced from this re-appearance agree in the most satisfactory manner with those calculated by M. Encke before the appearance of the comet, according to former observations, making allowances for irregularities produced by disturbing forces. Although this important calculation of M. Encke was well known the Academy which awarded to him, three years ago, the astronomical prize, founded by La Lande, we have thought it proper to mention the principal results, before we make known

what M. Damoiseau has added, in the memoir we have submitted to your examination.

The first part of this memoir was presented to the Academy the 16th of February last, and the second part the 26th of April. In the first part, M. Damoiseau, speaking of the elliptic elements calculated by M. Encke for the epoch of 1819, determines the perturbations which they have undergone from that epoch to 1822. The elements which he has deduced for this last epoch differ but little from those of M. Encke, which we have already cited; and it could not be otherwise, because, having both adopted the same data, the difference in their results must be owing to the difference in the methods they pursued. The duration of the anomalistic revolution of 1819 and 1822 being nevertheless known by observation, M. Damoiseau has made use of it in order to rectify the daily motion of 1819, of which he had already availed himself. He decided at the same time the mean daily motion of 1822, which was necessary to enable him to calculate the perturbations of the comet, from its first appearance to its next return to its perihelion. In this new calculation, M. Damoiseau makes use of the elements of 1822, deduced from the observations at Paramatta. The perturbations of this revolution are much less considerable than those that happened before, and the duration between two consecutive passages through its perihelion, will be about 1212 days like the preceding. M. Damoiseau fixes the next return of the comet to its perihelion, on Sept. 17, 1825. He gives for that epoch its elliptic elements, and he assigns the places which it will occupy in the heavens during the time of its appearance, but thinking that it will then be too near the sun to admit of observations being taken, he has proposed to calculate the epoch of its following return, and its ephemeris for that epoch. These calculations are the subject of M. Damoiseau's second memoir. The second return to the perihelion which he has determined, will take place Jan. 10, 1829, and the author announces that the comet, according to his ephemeris will be visible in every part of Europe. The time between this passage through the perihelion, and that of 1825 will be very nearly 1211 days, or only one day less than the period of the preceding revolution.

The second part of the memoir of which we have been speaking, contains, also the calculation of the perturbations of the comet from its appearance in 1805, to its return in 1819. Thus the entire interval of twenty-four years,

which the author has embraced in his calculations, comprehends three observed passages through the perihelion, and the two nearest returns to that point. The comparison of the three observed passages furnishes, for the epoch of each one of them, three values of mean daily motion, which differ but little from each other; the author has concluded for the last appearance upon the mean of these three values, by uniting with this the other elliptic elements calculated according to the observations at Paramatta; he has formed a table, which may be regarded as containing the definitive results of all his labours, and in which he has collected the elliptic elements answering to five passages through the perihelion, comprehended in the twenty four years which he had been considering. If we compare these elements, relative to the years 1805 and 1819, with those which are derived from actual observation made at those epochs, it will be seen that they agree in such a manner that the greatest deviations are but one minute in the longitude of the perihelion, five in that of the node, and two in the inclination of the orbit; but this comparison is not the best means of ascertaining the degree of exactness in the results obtained by M. Damoiseau. To judge of it properly it will be necessary that the author should calculate according to the elements, the places of the comet in these two epochs, in order to compare them with the places which have been directly observed.

M. Damoiseau has followed, in the calculation of the perturbations, the known method which consists in dividing the entire curve described by the comet, into portions, for each one of which we determine the effect produced by the disturbing forces of the planets. In the first part of his memoir, these portions answer to angles of 10° of eccentric anomaly. In the second part he has preferred to take intervals of time, nearly equal to each other, and each about thirty days; but in both cases, he is confident that he can, without any sensible error, observe in each portion of the orbit the variations of the mean motion, as proportional to the square of the time, and those of the other elements as proportional to its first power. However, we should observe that, if it were necessary to have regard to higher powers of this variable quantity, that the formulas which the author has given for this purpose, in the beginning of his memoir, would not be exact, and that it would be necessary to recur to the ordinary formulas of quadratures. Although the interval of

time comprehended between the appearance of 1805 and that of 1819, comprises four revolutions of the comet, M Damoiseau, in calculating the disturbances which have taken place in the course of that period, has always retained the same values of the primitive elements. It would nevertheless have been proproper to change them, at least every revolution, taking into consideration each time the preceding disturbances. This is a point which it will be sufficient merely for us to mention to the author if he proposes to review and complete his work.

In the period of twenty four years which the author has observed, he has constantly computed the action of Jupiter, and the action of the Earth and Venus only in that part of the period in which the comet is so near that this action is perceptible. As to the other planets, he has thought he might neglect their action, which he has regarded as imperceptible on account of their distance, or the smallness of their size. He thought moreover that he had no occasion to extend his caculations to the epochs of 1795 and 1786, in which the same comet had been observed, because he considered those observations not very exact, or not sufficient to determine the elements of the orbit. M. Encke on the contrary was desirous to represent these early observations; but he discovered that in the observed right ascensions and declinations, it would be necessary to suppose errors, the mean of which would amount to twenty four minutes, and this he judged to be inadmissible. To explain these differences between the results of calculation and observation, M. Encke thought that it would be necessary to recur to the resistance of ether, and he has announced in a late number of M. de Zach's Astronomical Correspondence, that by taking into account this tangential force, and making a suitable supposition in relation to its intensity, these great differences might be made to disappear, or at least be so far reduced as to be attributable to errors in the observations. There is no doubt that if the celestial bodies moved in a resisting medium, the effect of that resistance would be more sensible in the motion of comets than in that of the planets, as at the surface of the earth, we perceive that the resistance of the air has most effect upon the motion of those bodies having weight, whose density is least, and it actually is known that the density of comets is incomparably less than that of the planets; but if the comet, which has been the subject of consideration is destined to exhibit to us in the

the heavens the effects of a force, of the necessity of which no other phenomenon has ever yet made us sensible, it must require a greater number of revolutions of that body to establish with any probability so delicate a point in astronomical science.

The calculations, to which M. Damoiseau has devoted himself, with a zeal worthy of all commendation, have already given with a great degree of exactness, the motion of the comet of 1819. The same philosopher has likewise calculated the next return of the comet of 1759 to its perihelion, and the academy of Turin has rewarded him for this laborious task by decreeing him the prize which had been offered on that subject. Yet the importance of these questions has made it appear desirable to you that the theories of these two comets should be made more perfect, and in proposing, at your last meeting, the general theory of the perturbations of comets as the subject of the prize of 1826, you have mentioned as particular applications, the motion of these two comets, so different from each other in the duration of their revolutions. We unite with you in the hope that geometers and astronomers, not connected with the Academy, will answer to your appeal, but in the mean time we must not do the less justice to the researches of M. Damoiseau. We think that they decree your approbation, and we would propose to you to direct that the Memoir of which we have given you an account, be printed in the *Recueil des Savans étrangers*. This report and its conclusions were approved by the Academy in the session of 24th May, 1824.

ART. XV.—*On the Influence of Solar Light on the Process of Combustion.* By THOMAS M'KEEVER, M. D. (Abridged for this Journal from the Ann. Phil. by H. D. Ward.)

THE popular opinion that the sun's rays falling directly upon a common fire, have the effect of retarding the progress of combustion, induced Dr. M'Keever to institute several experiments, with a view to determine its accuracy. And we are happy to find another proof of the position, that opinions generally enter-

tained among men, do commonly rest more or less on experience and observation.

Exper. 1.—Two portions of green wax taper, each weighing ten grains, were both ignited at the same moment. One was placed in a darkened room, the other exposed to broad sunshine in the open air. Thermometer in the sun, 78° Fahr. ; in the room 67°.

In five minutes that placed in the sunshine lost $8\frac{1}{2}$
Darkened room, - - - - - - - - - - - - $9\frac{1}{4}$*

Exper. 3.—A common mould candle, fourteen inches in length and three in circumference, was accurately divided into inches, half inches and eighths, and exposed in the first instance to strong sunshine. Thermometer 89° Fahr. ; atmosphere remarkably calm :—to consume one inch it took 59′0″
In darkened room, (Temp. 68° F.), - - - 56′0″
In ordinary daylight, (Temp. 68° F.), - - 57′10″

Exper. 5.—Two lanterns, one coated with black paint and the other naked, were each furnished with a portion of taper of precisely equal weights, and both exposed to the full glare of sunshine. In ten minutes

that in painted lantern lost .$16\frac{1}{2}$ grains.
that in uncoated lantern, - 15 grains.

Dr M'Keever, by varying the last experiment, sought also the results of combustion in the full light of the moon, but could detect do difference between the combustion of the tapers in the different lanterns, whether in moonlight or in a darkened room.

In reasoning upon these results, says Dr M'Keever, it occurred to me that they were probably owing to the well known decomposing power possessed by the solar rays, in consequence of which the shell of air that immediately encircles a particle of matter about to enter into combustion, is deprived, to a certain extent, of its oxygenous principle ; and is thus rendered less fitted for the maintenance of this important process.

As the solar rays are now ascertained to consist of three distinct species of radiant matter, viz. those that impart heat, those that impart light, and the chemically acting rays, it appeared to me that the best mode of putting this conjecture to the test of experiment, would be to try whether any difference could be detected in the loss sustained by a lighted taper when exposed to the several portions of the solar spectrum.

I allowed the rays of one colour only to pass at a time through an opening made in a piece of pasteboard, the room

* Experiments two and four only confirm the general result, and are omitted in this abstract.

being darkened, and the prism operating by means of an opening in the window shutter. A piece of green taper accurately marked after the usual manner, was now ignited, and submitted to different portions of the spectrum, with the following results. To consume two inches of taper in the

red ray, it took - - - - - - - -	8′ 0
green ray, - - - - - - - -	8′ 20″
violet ray, - - - - - - - - -	8′ 39″
verge of violet ray, - - - - -	8′ 57″

Commencing with the violet ray, it took to consume one inch at the

verge of the violet ray. - - - - -	4′ 36″
in the centre of the violet ray, - -	4′ 26″
green ray, - - - - - - - - - -	4′ 20″
red ray, - - - - - - - - - -	4′ 16″

This short enquiry then, although perhaps, not conducted with all that delicacy and precision so necessary for strict philosophical research, would apppear to warrant two conclusions of some interest. First, that the solar rays, in proportion to their intensity, are possessed of the power of retarding, to a considerable degree, the process of combustion ; and that consequently, the popular ideas on this subject are founded in truth. Secondly, that this phenomenon is occasioned by the action of the chemical rays on the portion of the atmospheric air, that immediately envelopes a particle of matter about to enter into a state of combustion, aided no doubt by the high temperature of the portion of materials that have already commenced this process.

ART. XVI.—*An attempt to establish the first Principles of Chemistry by Experiment.* By THOMAS THOMSON, M. D. F. R. S. Regius Professor of Chemistry in the University of Glasgow, 2 Vols. 8vo. Lond. 1825. [W.]

WHILE chemistry invites the philosopher to explore the wonders of nature, and unfold the laws regulating its mysterious processes, it holds out inducements, less ennobling it is true, but not less powerful, to the artist and manufacturer, in the hope it inspires of increasing his pecuniary advantages. Besides the number of general students it has drawn from the retreats of classical literature, and the abstruser sciences, a crowd of

votaries have appeared from the more busy walks of life. It has created a new population of scientific artists and practical philosophers, who are to be sought in the workshop and in the manufactory.

In those countries where the arts have attained a high degree of perfection, there are probably few men who aim at precision in their processes, that do not become students of chemistry, and who do not follow with lively interest, every discovery in that science which may in any way affect their peculiar occupations. The multitude of scientific journals, elements, dictionaries, and systems of chemistry which the press is continually pouring forth, and to which a valuable addition has been made in the work of Dr Thomson, indicate the spirit which is abroad, and the increassing multitude of scientific readers. Although a difference of opinion may exist among chemists, in regard to some of the views of Dr Thomson, we cannot but think they will agree in considering this as the most elaborate and original work on chemical philosophy which has appeared for many years.

Dr Thomson's volumes abound in new, ingenious, and delicate experiments, the object of which, was to determine the atomic weight of bodies; or, in the language of the author, to reduce the whole doctrine of atoms to the utmost degree of simplicity and accuracy, so as to put it in the power of the chemical manufacturers in general, to avail themselves of it in order to bring their processes to the requisite degree of precision.

Whatever degree of hypothesis may have been blended with the discussions and views in which the atomic theory is concerned, it has now attained a value and importance far beyond what its early advocates could have anticipated. Among other advantages, it is conceived by Dr Thomson, that a knowledge of the atomic weights of bodies, and of the weights of the integral particles of the salts, will be found highly useful to medical practitioners. *It will enable them "to avoid some very awkward* blunders, into which too many, even of considerable celebrity, frequently fall, to the no small inconvenience of their patients. Among chemists too, remarkable examples could be pointed out, of men who would take it amiss if the world did not give them credit for possessing much chemical science, falling into mistakes, that no student who had followed and understood the lectures on chemistry, delivered at the College of Glasgow, would run any hazard of committing."

The publication of the present work has shown that Dr Thomson has not been content with the faithful performance of the prescribed duties of his office, but with indefatigable zeal,

has devoted no small portion of his "leisure" hours, to private and laborious investigation. The results which are contained in these volumes, will speedily become known wherever science is cultivated, and redound not less to the honor of the University, than to that of its distinguished professor.

Dr Thomson's work contains the results of many thousand experiments, which have occupied the whole of his time, except what was necessarily devoted to the duties of his situation, for the last five years ; an appropriation of time which may, to those unacquainted with the subject, seem unnecessarily large ; but which is fully warranted by the importance of the subject, and the necessity of obtaining accurate results. Those who are not practically conversant with the minute details of experiments like these, and who have not watched the progress of chemical science, can have but very imperfect notions of the degree of accuracy and precision, which its processes have acquired, and of the amount of time they consume. The processes of chemistry have arrived within a few years, at such a degree of precision and accuracy, and so much care is now bestowed upon experiments, that we may rely upon the results within less than one per cent. I believe, indeed, says Dr Thomson, that the error in many of Berzelius' analyses does not exceed one thousandth part.

As a specimen of the manner in which the experiments of Dr Thomson were conducted, we shall select a portion of the section relating to the composition of zinc. As it had been demonstrated by Gay-Lussac and Humboldt, that water is a compound of oxygen and hydrogen gases, in the proportion of one volume of the former to two volumes of the latter, it occurred to Dr Thomson that by a very careful analysis of water, the relation between the volumes and the weights of these gases might be determined. These experiments were made by means of zinc, and it therefore became highly important to know the exact composition of the oxide of that metal. The zinc of commerce is never perfectly free from carbon, and small quantities of other metals ; that

The zinc employed by Dr. Thomson "was obtained by exposing common zinc to a white heat in a stone-ware retort, luted to a receiver nearly filled with water. At this temperature, the zinc is sublimed, and freed from all its impurities, except a trace of cadmium too minute to occasion any sensible error in the result of the experiments. The zinc thus distilled over, should be melted in a crucible, and poured upon the surface of a clean, smooth sandstone. We thus obtain it in a thin sheet

which can be easily broken into small pieces to suit the object in view.

21·25 grains of this zinc were introduced into a small green glass retort, the weight of which had been previously determined. As much very weak but pure nitric acid as was fully sufficient to dissolve the whole zinc was introduced into the retort. As soon as the metal was all dissolved, a receiver was applied to the retort which was placed in hot sand, and allowed to remain till the whole liquid contents had distilled over. The liquid in the receiver was saturated with carbonate of ammonia, and boiled for some time to see whether it contained any zinc; but no precipitate appeared.

The retort, thus freed from its liquid contents, was placed upon a charcoal fire, and kept for about an hour in a dull red heat. This completely decomposed the nitrate of zinc, which existed at first in the retort, dissipated the nitric acid, and left nothing behind but the 21·25 grains of zinc, now converted into an oxide. Being placed in the scales, the zinc, by being oxydized, was found to have increased in weight 5 grains. Thus, it appears that oxide of zinc is composed of

Zinc	21,25
Oxygen	5
	26,25

or, dividing each constituent by 5 to bring them to their lowest terms, of

Zinc	4,25
Oxygen	1
	5,25

As zinc combines with oxygen in only one proportion, it is reasonable to conclude that the oxide is a compound of 1 atom zinc and 1 atom oxygen. If this supposition be admitted, the atom of zinc is obviously $4\frac{1}{4}$ times heavier than the atom of oxygen. And if we represent the atomic weight of oxygen by unity, then the atomic weight of zinc will be 4·25, and the integrant particle of ozide of zinc will weigh 5·25; or in other words, 5·25 of oxide of zinc will just saturate an integrant particle of any acid with which it is capable of combining. The law holds with all acids; but I shall at present verify it only by referring to three acids; namely, the sulphuric, carbonic, and arsenic acids. It will be shown in the 5th, 1st, and 7th sections of the sixth chapter of this work, that the atomic weights of these acids are as follows:

Sulphuric acid	5
Carbonic acid	2,75
Arsenic acid	7,75

Now, 5·25 of oxide of zinc will just saturate 5 of sulphuric acid, 2.75 of carbonic acid, and 7·75 of arsenic acid; or in other words, the sulphate, carbonate, and arseniate of zinc, supposing them anhydrous, are composed as follows:

1. SULPHATE OF ZINC.

Sulphuric acid	5
Oxide of zinc	5,25

2. CARBONATE OF ZINC.

Carbonic acid	2,75
Oxide of zinc	5,25

3. ARSENIATE OF ZINC.

Arsenic acid	7,75
Oxide of zinc	5,25

We shall verify this statement by a rigid analysis of these three salts.

1. SULPHATE OF ZINC.

Sulphate of zinc, or white vitriol, is a well known salt which crystallizes in beautifully transparent four-sided prisms, terminated by four-sided pyramids; or if the crystallization be rapid, it assumes the form of long slender white needles. However carefully prepared, it always reddens vegetable blues. But it crystallizes also with an excess of acid. In that case, the crystals assume the form of large flat rhomboids, which are not altered by exposure to the air, and may be raised to the temperature of at least 500° without being deprived of their excess of acid.*

18·125 grains of the neutral sulphate of zinc were dissolved in distilled water, and precipitated by a solution of carbonate of soda. The precipitate was separated from the liquid by means of a double filter, consisting of two pieces of unsized and well washed printing paper, made previously of the same weight. The carbonate of zinc on the filter was washed with distilled water, till the liquid which passed through ceased to render mu-

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* I am doubtful whether this salt constitutes a truly distinct species. I found its composition.

$1\frac{1}{3}$ atom acid	6,66
1 atom oxide	5,25
8 atoms water	9,00
	20,91

or, it consists of one atom of the potrohydrate of sulphuric acid, combined with three integrant particles of the neutral salt.

riate of barytes muddy. The filter was suspended in a linen bag above the sand bath, in a temperature of 85°, till it was apparently dry; and it was finally left for some time in a temperature of about 212°. It was afterwards weighed by placing the two filters in opposite scales, and adding weights till the two scales were exactly counter poised. The weight of the carbonate of zinc was exactly 8 grains. It was put into a platinum crucible, which was covered with a lid, and exposed for half an hour to a red heat. There now remained 5·25 grains of oxide of zinc.

Thus it appears, that 18·125 grains of sulphate of zinc, in crysals, contain 5.25 grains of oxide of zinc. It is still easier to prove that the sulphuric acid contained in this quantity of the salt weighs precisely 5 grains.

But it will be necessary to state, in the first place, that when 15·5 grains of crystallized muriate of barytes are exposed to a red heat, they lose 2·25 grains of their weight, and are converted into 13·25 grains of chloride of barium. When 13·25 grains of chloride of barium are dissolved in water, the solution contains just 9·75 grains of barytes. Now, 9·75 represents the atomic weight of barytes, which requires for saturation exactly 5 of sulphuric acid. And 14·75 parts of sulphate of barytes contain just 5 parts of sulphuric acid.

A little distilled water was put into two small cylindrical glass jars, and 18·125 grains of crystallized sulphate of zinc were dissolved in the one of these, and 13.25 grains of chloride of barium in the other. The two liquids being mixed, a double decomposition took place, and sulphate of barytes precipitated in the state of a white powder. This precipitate being collected, washed, dried, and heated to redness, weighed 14.75 grains, and therefore contained exactly 5 grains of sulphuric acid. A few drops of the colourless liquid from which this sulphate had precipitated, were put into a watch glass, and a small quantity of muriate of barytes mixed with it; but it did not become in the least muddy or even opal coloured, showing that it contained no sensible quantity of sulphuric acid. For I find that 1 grain of glauber salt (containing 0.2469 grain of sulphuric acid) dissolved in 12,000 grains of water, is sensibly precipitated by muriate of barytes. Hence it is clear, that in the present experiment, the quantity of sulphuric acid remaining in the liquid could not amount to $\frac{1}{4052}$ of a grain. Indeed, as the quantity of liquid never amounted to 1000 grains, the sulphuric acid remaining in solution, could not be so much as $\frac{1}{48624}$ of a grain.

Another portion of this residual liquid being mixed with sulphate of soda, was not in the least altered, showing that the liquid contained no sensible portion of barytes. Thus it appears, that the barytes from 13.25 grains of chloride of barium, or 15.5

grains of muriate of barytes, just saturate the sulphuric acid in 18·125 grains of sulphate of zinc. Hence, this acid just weighs 5 grains.

We see therefore, that in neutral sulphate of zinc, 5.25 parts of the oxide are combined with 5 parts of the acid : which was the thing to be proved. These two constituents added together make 10.25, which subtracted from 18.125 leave 7.875 for the quantity of water of crystallization. It will appear afterwards that this is equivalent to 7 atoms of water. Sulphate of zinc in crystals, then, is composed of

1 atom sulphuric acid	5
1 atom oxide of zinc	5,25
7 atoms water	7,875
	18,125

So that 18.125 is the weight of an integrant particle of the crystallized salt. It was the knowledge of this that induced me to make choice of 18.125 grains of the salt. My first experiments were made upon a much larger quantity ; but I have often repeated them with 18.125 grains, and obtained the result just stated.

If the solution of neutral sulphate of zinc be concentrated too much, it deposites, while still hot, an opaque crust upon the bottom of the vessel. This crust is very white, not so soluble in water as the crystals, and has not the least appearance of crystallization. It is neutral, though it reddens vegetable blues like the preceding salt ; and being analysed in the same way as the crystallized sulphate, was found composed of

1 atom sulphuric acid	5
1 atom oxide of zinc	5,25
3 atoms water	3,375
	13,625

So that an integrant particle of it weighs 13.625, and it differs from the crystals in containing only 3 atoms of water, instead of seven."

In the introductory part of the present work, Dr Thomson has given an historical sketch of the developement and progress of what has been termed the Atomic Theory ; of this we shall now offer an abridgment with additional remarks, from Beudant and the papers of other writers scattered through the various scientific journals.

From the time that bodies were conceived to be constituted

of simple elements, it was believed that those elements were combined in certain proportions. The first accurate experiments on the composition of one class of bodies, viz. the salts, were made by Wenzel, a German chemist who published a small work in the year 1777, entitled *Theory of the Affinities of two Bodies*. He had observed that when two neutral salts mutually decompose each other, the resulting compounds are also neutral; and that the relative proportions of alkalies and earths which saturate a given quantity of the same acid, are the same for all other acids. If nitrate of lime for example, be decomposed by sulphate of potash, the nitrate of potash and sulphate of lime which result, preserve their neutrality, for the relative quantities of potash and lime which saturate a given weight of water and sulphuric acid are the same.

Bergman also bestowed much attention upon the determination of chemical proportions, and explained the phenomena noticed by Wenzel in the same way. Berthollet subsequently endeavoured to prove that the elements of bodies have certain fixed points, a maximum and minimum, beyond which they will not enter into combination, but that between these limits they unite in any proportion. The phenomena noticed by Wenzel were likewise observed by Richter, who endeavoured to determine the relative capacity of saturation of the acids and bases, and attached a number to each indicating this capacity. The little attention which the labours of this chemist attracted, is ascribed by Berzelius to two causes, viz. 1. The great inaccuracy of Richter's experiments. 2. The violent discussion between the antiphlogistic and phlogistic system, which originated about the year 1787.

The opinions of Bergman were contested by Proust, who demonstrated that the combinations which metals form with oxygen and with sulphur, are in fixed and invariable proportions, and that the supposed intermediate compounds are merely mixtures of two definite oxides or sulphurets.

In the year 1789, Mr Higgins published his "Comparative view of the Phlogistic and Antiphlogistic Theories," in which he maintains that when gases combine in more than one proportion, all the proportions of the same element are equal. This idea was founded on the corpuscular hypothesis, that bodies combine particle with particle, or with one, with two, or three, or a greater number of particles."* He does not appear, however, to

* Davy's Elements.

have prosecuted the enquiry, but it was left for the genius of Dalton to give a much more general view of the subject, which has been usually called the atomic theory. Dr Thomson conceives that the views upon which this theory is founded, occurred to Mr Dalton in consequence " of his own experimental investigations," an opinion which though not subscribed to by all chemists, yet derives some support from the remark of Sir H. Davy, that Mr Dalton had apparently no knowledge of what Mr Higgins had written.* The account which Dr Thomson gives of his first knowledge of the views of Mr Dalton is as follows.— " I first visited him at Manchester on the 26th of August, 1804, and on that day he explained to me his notions respecting the composition of bodies. The ultimate particles of all simple bodies, are in his opinion *atoms* incapable of further division. These atoms (which Mr Dalton supposes to be surrounded by atmospheres of heat,) are all spheres, and are all of them possessed of particular weights which may be denoted by numbers. And he represented the atoms of the simple bodies by symbols."

In 1807, Dr Thomson, published a sketch of this hypothesis in the third edition of his system of chemistry. In 1808, Mr Dalton published the first volume of his new system of chemical philosophy. The second volume appeared in 1810, in which he examined the combinations of oxygen with hydrogen, azote, carbon, sulphur and phosphorus ; and various other combinations which he showed to be in accordance with the atomic theory. The third volume of this important work, Dr Thomson remarks has been printed for several years, but the author has thought proper to withhold it from the public.

The views of Mr Dalton have received great support from the experiments of Dr Wollaston and others. Berzelius has remarked that this discovery may be considered without exaggeration, as one of the greatest steps that chemistry has ever made towards perfection, and that the honour of the invention of the doctrine of *multiple proportions* is due to Dalton alone.

In order to express the relative weights of the atoms of bodies, it was found necessary to assume the atom of some body as a standard or unity. Mr Dalton made choice of the atom of hydrogen, and in this he has been followed by Davy, Brande, Dr Henry, and various other chemical writers. Other chemists have assumed oxygen as the unit, and among these is Dr Thomson, who assigns the following reasons.

" It is obvious, that the ratios are much more easily observed,

* Elements.

and much more easily employed, the smaller the numbers are, by which they are denoted. Thus the ratio of 39841 to 116223 is the same as that of 1 to 3. But how very inconvenient would the great number of figures be, compared with the simple number 1 : 3. It is therefore highly important in chemistry, that the atomic weights of bodies should be represented by the smallest possible numbers. Now, when we make the atom of oxygen unity, the atomic weights of all bodies are represented by numbers eight times less than they would be if the atom of hydrogen be unity. It has been represented by some persons, in favour of the atom of hydrogen being unity, that in that case the atomic weight of every body is a whole number ; whereas, when oxygen is the unity, fractions are frequently unavoidable. And these fractions, it is said, render the atomic weights more complicated and unmanageable, than the greater size of the whole numbers deduced from hydrogen as a unity. But this view of the subject is quite erroneous. It will be shown in this work, that even when oxygen is made unity, nearly one half of all the atomic weights of bodies are whole numbers. And when fractions exist, they are always either 0.25, 0·5, or 0.75 ; except in four or five cases, when the fraction is 0·125, 0·375, or 0·625. If hydrogen be unity the atom of manganese is 28, while it is $3\frac{1}{2}$ if oxygen be unity. The atom of barytes on the former supposition is 78, on the latter $9\frac{3}{4}$. Now surely it will not be said that the fractional numbers are more unwieldy, or more unmanageable than the whole numbers ; while in all cases of whole numbers the advantage on the side of the latter method is very great. Thus, if hydrogen be unity, the atom of uranium is 208, while if oxygen be unity it is only 26."

The next step in the progress of the atomic theory is to be found in the researches of Gay-Lussac and Humboldt, who found that one volume of oxygen gas combines with two volumes of hydrogen gas to form water. The first of these two philosophers some time after, found that gases in general unite with each other in a very simple manner ; one volume of one gas, either combining with one volume, with two, or with half a volume of the other ; or in other words, that gases either combine in equal volumes, or the volume of one gas is a multiple by a whole number of that of the other. It has been remarked by an eminent chemist, that if we substitute the word *atom* for that of *volume*, and imagine the substance to be in the solid instead of the gaseous state, Gay-Lussac's theory coincides exactly with Dalton's, and is a direct confirmation of its truth. But, says Dr Thomson, Mr Dalton, instead of welcoming the papers which contained the discovery of Gay-Lussac, as an important addition to his own views, set himself to show that Gay-Lussac's opinions

were ill founded. The subsequent researches of chemists, however, "have left no doubt about their accuracy; and if Mr Dalton still withholds his assent, he is, I believe, the only living chemist who does so."

Dr Thomson, at page 19, has taken a very interesting notice of Professor Berzelius, of Stockholm, who has done so much for the establishment of the atomic theory. To him are we indebted for a far greater number of accurate analyses, than to the whole chemical world put together. This indefatigable chemist has embraced every department of chemistry with equal ardour, and to such a degree of accuracy has he attained, that as already remarked, it is believed the error in many of his analyses does not exceed one thousandth part.

As the views of Berzelius are peculiarly interesting, we shall avail ourselves of the notice lately taken of them by Mr Children,* reserving any further account of Dr Thomson's work for a future number.

When we reflect on the cause of chemical proportions, the most probable idea that presents itself to our imagination is, that all bodies are composed of elementary particles or atoms, incapable of mechanical division,† and which unite together in such a manner, that an atom of one element combines with 1, 2, 3, &c. atoms of another element. With this simple postulate, to which the mind readily accedes, it is easy to explain all the phenomena of chemical proportions. By the union of two or more elementary atoms, compound atoms are formed which are as incapable of mechanical division as the former, and these again unite together to form still more compound atoms, and so on to the most compound. Berzelius divides these several combinations into separate orders—the simplest, or the atoms of the first order, are composed of simple elementary atoms, and are either inorganic, or organic. The former contains only two elements, the latter always three at least. Compound atoms of the second order are produced by the union of compound atoms of the first order; those of the third by the union of atoms of the second order, &c. Thus sulphuric acid, potash, and water, are compound atoms of the first order, since they consist only of a base and oxygen. Sulphate of potash and sulphate of alumina are atoms of the second order, being formed by the union of compound atoms of the first order. Anhydrous alum, which is composed of the two last mentioned salts, belongs to the third order; and crystal-

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* Annals of Philosophy.

† On the subject of the limited divisibility of matter, see Dr Wollaston's beautiful paper, "On the Finite Extent of the Atmosphere."—(Phil. Trans. for 1822, pp. 39.)

ized alum, which besides those elements, also contains several atoms of water, is an instance of a compound of the fourth order.

"But taking it for granted that bodies are composed of indivisible atoms, certain laws must regulate their combinations, in order to establish them in definite chemical proportions, and on which the constancy of those proportions must depend. The existence of such laws and the probable manner in which elementary atoms combine, is inferred from experiment, which teaches us that

"1. *An atom of one element combines with one, two, three, or more atoms of another element.* This is of most frequent occurrence, so that in the greater number of compound atoms, one of the elements enters only as a single atom.

"2. *Two atoms of one element combine with three atoms of another element.* This may happen whenever the quantity of oxygen in a protoxide is to that in the deutoxide as $1 : 1\frac{1}{2}$; as is the case with iron. If the first oxide be composed of an atom of base united to an atom of oxygen, the second must contain two atoms of base united to three atoms of oxygen; for there can be no such thing as half an atom. This apparent anomaly may be reconciled in a different manner, by supposing that there may be another hitherto unknown oxide of iron, containing half the quantity of oxygen that is contained in the lowest of the two known oxides. According to this view, an atom of base in the unknown oxide is united to one atom of oxygen, and in the two known oxides to two and three atoms respectively. Dr Thomson with Berzelius adopts the former solution.

"In the compound atoms of the second order, the ratio of two atoms of one element to three of another is found less equivocally, though the instances are rare. Thus the hydrated red oxide of iron is composed of two atoms of oxide combined with three atoms of water; the subsulphate of copper contains two atoms of acid and three atoms of base. Unless we consider these as the true proportions of the elements of the above compounds, we must suppose the oxide of iron, as well as the sulphuric acid, to contain six atoms of oxygen; but till new facts show the probability of that supposition, there is sufficient ground for considering the ratio to be that of 2 : 3. There is no reason for supposing that two atoms of one element combine with four, five, six, or a greater number of atoms of another element, but in the varied productions of the mineral kingdom, we find compounds somewhat different from those we can produce in our laboratories. Amongst the silicates, combinations in which

three compound atoms of the first order are united to four of the same order are frequent, as in laumonite, amphigene, &c. ; but in our artificial productions, analogous cases are extremely rare.

" As a general conclusion, we may assume, that in inorganic compounds the simple atoms combine in very limited proportions ; the most common is that of one atom of one element with one or more atoms of another element, so that in most compounds, one of the elements may be represented by unity : the next most common proportion is that of two atoms of one element to three of another ; and in the mineral kingdom, in the compound atoms of the third and fourth orders, we sometimes meet with three atoms of one body united to four atoms of another body.

" There is another law which according to Berzelius, regulates the combination of compound atoms of the first order ; namely, that oxidated bodies always combine in such proportions that the number of atoms of oxygen in one of them is a multiple by a whole number of the number of atoms of oxygen in the other ; and in like manner, in the combinations of sulphuretted bodies, the sulphur in one is a multiple by a whole number of the sulphur in the other. Or, to express the law in general terms, compound atoms of the first order, having a common electro-negative element, combine in such proportion that the electro-negative element of one atom is always a multiple by a whole number of the electro-negative element of the other.

" The only known exceptions to this law are the acids of phosphorus, nitrogen and arsenic, which combine with other oxidated bodies in such proportion that the number of atoms of oxygen in the oxide is one or more *fifths* of the number of atoms of oxygen in the phosphoric, nitric, and arsenic acids, and one or two *thirds* of the same number in the phosphorus, nitrous and arsenious.

" When two salts, having a common acid but different bases combine, the number of atoms of oxygen in one of the bases is a multiple by a whole number of the number of atoms of oxygen in the other ; and consequently, the acid in one of the salts is a multiple by a whole number of the acid in the other. In alum and feldspar, the number of atoms of oxygen in the alumina is triple the number of atoms of oxygen in the potash ; and in like manner the quantity of sulphuric acid and of silica, respectively combined with the alumina, is triple that combined with the potash. In the double tartrate of potash and soda, the two alkalies contain the same number of atoms of oxygen, and are consequently combined with the same number of atoms of tartaric acid.

"When two salts, having a common base but different acids combine, the number of atoms of oxygen of the portion of the base combined with one of the acids is a multiple by a whole number of the number of atoms of oxygen in the portion of base combined with the other acid; or, the number of atoms of oxygen in one of the salts is a multiple by a whole number of the atoms of oxygen in the other. We have an instance of this species of combination in Datholite, a compound of borate and silicate of lime, in which the lime is equally divided between the two acids; whilst in the blue carbonate of copper, the quantity of base combined with the carbonic acid is twice as great as that combined with the water.

"In organic bodies, the compound atoms of the first order contain at least three elements, oxygen, hydrogen, and carbon, and their atoms are capable of combining in all sorts of proportions, so that neither of them can be taken as unity with reference to the rest. But when organic atoms of the first order combine with compound inorganic atoms of the first order, as when a vegetable acid combines with an oxide, they follow the same laws that regulate the combinations of the compound inorganic atoms, and the oxygen of the organic atom is a multiple, or sometimes a submultiple, by a whole number of the oxygen of the compound inorganic atom. Hence these combinations present the same phenomena of definite proportions, as those of inorganic nature, and it is only in the formation of organic atoms of the first order, that combination is possible in all sorts of proportions.

"In the earliest experiments with the voltaic pile, it was found that the common salt, a solution of which was usually employed to moisten the pasteboard discs interposed between each pair of metallic discs, became decomposed by its action; and about the year 1805, MM. Hissinger and Berzelius published their experiments on the galvanic decomposition of the muriates of ammonia and lime, and several other neutro-saline solutions.

"The results of those experiments demonstrated that when electricity traverses a saline solution, or any other liquid conductor, its elements are separated in such a manner, that some collect round the positive, and others round the negative pole; and that oxygen, acids, and oxidated bodies terminate to the former, whilst combustible bodies, alkalies and the earths, find their point of rest at the latter.

"On the 20th of November, 1806, Sir Humphrey (then Mr) Davy, read his Bakerian lecture on some chemical agencies of electricity, before the Royal Society, which was afterwards pub-

lished in the Philosophical Transactions for the following year. In this celebrated paper which obtained the prize offered by Napoleon Bonaparte, for the best essay on voltaic electricity, Sir Humphrey Davy clearly promulgated, for the first time, the laws by which the chemical agencies of electricity are regulated, and the principles on which their power of suspending or destroying the usual order of chemical affinities depends. He showed that dfferent elementary substances have different electrical energies, some being naturally positive, and others negative, with respect to each other ; and that when a compound formed of two such elements, is decomposed by the voltaic battery, the body possessing positive energy is repelled by positively electrified surfaces, and attracted by negatively electrical surfaces ; and that the body possessing the negative energy follows the contrary order.

" Adopting these views, Berzelius divides all substances into two great classes, the electro-positive and electro-negative. Simple bodies belonging to the former class, as well as their oxides, always assume the possitive state when they meet with other simple bodies or their oxides belonging to the latter ; and the oxides of the first class bear the same relation to those of the second, that salifiable bases bear to acids. He considers oxygen as the most electro-negative of all bodies, and the only one whose electrical relations are invariable, it never being positive with respect to any other, and he places it accordingly at the head of his table exhibiting the supposed order of elementary substances with respect to their electrical relations. The last substance in the table, and consequently the most possitive, is potassium, and all the intermediate substances between oxygen and that body are considered as negative to all those which stand below them, and positive to all that stand above them in the table.

" Long before any idea had been formed of the electrical relations of simple combustible bodies, their oxides were divided into acids and bases, the first forming the electro-negative, the second the electro-positive class ; amongst the bodies of the first class a weak acid often serves as base to a more powerful acid, and in the electro-positive series, a weak base frequently acts as an acid with respect to one more strongly electro-positive.

" The electrical relations of oxides usually depend on those of their bases ; thus an oxide is electro-negative to another oxide, if the base of the former be negative with respect to that of the latter, and *vice versâ*. Sulphuric acid for instance, is electro-negative with respect to all the metallic oxides, because sulphur is negative with respect to all the metals ; the oxides of potas-

sium and zinc, on the contrary, are electro-positive in regard to all oxidated bodies, with respect to whose bases potassium and zinc are positive. Hence acidity does not depend, as the antiphlogistic theory assumes, on oxygen as the acidifying principle, but rather resides in the radical of the acid, and the oxygen enters indifferently into the most electro-positive and electro-negative bodies, or the strongest bases and acids.

"When the two electricities are separately manifested in any body, they are concentrated in two or more opposite points or poles, analogous to the poles of a magnetic needle. This electrical polarity of which the tourmaline furnishes a striking instance, must also belong to the minutest particles of the body, as well as to the whole mass; and this affords an easy solution for all the phenomena of electro-chemical affinity; for the different forces with which elementary bodies unite may be conceived to depend on the different intensities of the electrical polarities of their atoms. This hypothesis, however, is not sufficient to explain why some bodies are *electro-positive* and others *electro-negative;* but if we imagine the atoms of such bodies to possess *unipolarity*, similar to that which Ehrman observed in various substances, we may conceive that in the atoms of electro-positive bodies, the electricity of the positive pole predominates, and in those of electro-negative bodies, that of the negative pole, and consequently these bodies will always be in opposite states with respect to each other.

"The degree of affinity between different bodies cannot however depend wholly on their specific unipolarities, but rather on the intensity of the polarities generally; for oxygen and sulphur, both electro-negative substances, combine with much greater energy than oxygen and copper, although the latter is an electro-positive element; and the influence of temperature has a powerful effect in modifying attractions.

"If these conjectures," says Berzelius, "present a correct idea of the relations of bodies to electricity, it follows that what we call chemical affinity is nothing else than the effect of the electrical polarity of their atoms, and that electricity is the primary cause of all chemical action."

But it is time to quit this part of the subject, and turn to that which is more immediately the object of this abstract; and first, we shall endeavour to explain the manner in which the relative weights of the atoms of bodies are determined.

It is obviously necessary for this purpose, that some substance should be fixed upon, the weight of whose atom may be assumed as unity; Dalton chose hydrogen for his unit, as the sub-

stance of which the smallest weights enter into combination: He has been followed by Davy, Brande, Henry, Phillips, and various other writers; whilst Wollaston, Thomson, and Berzelius adopt oxygen as their lowest number, that substance being of all others most universally present in inorganic bodies. On the scale of chemical equivalents, Dr Wollaston reckons oxygen as ten, Thomson considers it as one, and Berzelius as one hundred. It is of small consequence which atom be selected for the purpose, or what relative value be assigned to it, whether ten, one, or one hundred; but whichever be chosen, the weights of the atoms of all other bodies must be exressed in some function of that unite.

[*To be continued.*]

ART. XVII. *Abstract of a Memoir accompanying Meteorological Observations made at Washington, D. C. in the years* 1823--4. By JULES DE WALLENSTEIN, Esq. Corresponding Member of the Academy of History of Madrid.* [*Translated and abridged for this Journal.*]

THE barometer which M. Wallenstein uses in his observations at Washington was constructed by M. Fortin, a skilful artist, who has enriched the Royal Observatory at Paris, with an excellent circle, and who was before known for the extreme exactness of his balances and other instruments which demand equal skill in their construction. The cistern of this barometer has a moveable bottom: an ivory point indicates the moment when the surface of the mercury in the cistern is at its true level. The instrument is so contrived that the height of the barometric column can be ascertained to the tenth of a millimetre, or the four thousandth of an English inch. It is screwed in such a manner in its upper part, as constantly to preserve its verticality. A centigrade thermometer is enchased in its frame.

The hygrometer of Saussure is 2,5 decimetres long, and the arc is divided into 100°. The extreme point of dryness is reduced by calculation, to zero of the scale. A centigrade thermometer constructed by Fortin, is suspended by the side of this instrument, and they are both enclosed in a small case, which preserves them from the dust, the two opposite sides of which are covered with a gauze sufficiently fine to leave them in perfect contact with the

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* Written in French and originally published in the Transactions of the American Philosophical Society of Philadelphia, vol. ii. N. S.

atmosphere : These instruments are exposed to the west : they are suspended within 2,4 metres of the earth.

In observations purely thermometric, M. Wallenstein uses two thermometers, graduated on the tubes themselves ; (one constructed by M. Lerebours, an artist attached to the Bureau des Longitudes de Paris, the other by M. Fortin ;) and a thermometer of Mr Troughton's, with the centigrade scale and Fahrenheit's.

As most of M. Wallenstein's instruments are divided according to the metrical scale, he has reduced each observation to the English measure, in order to render his observations more easily comparable with those which have been made in other parts of the country.

He follows the rules of M. Biot respecting the correction for capillary attraction, and for the influence of temperature on his barometric observations. The interior diameter of the tube of his barometer is 9,5 millimetres. He applies a correction of 4,893 millimetres to each of his observations, according to the table of depression calculated by La Place. When this correction is added, the observations are the same as if they had been made with a tube, so large that the mercury could not sink below its true level, as is the case in capillary tubes.

To apply a correction for the temperature of the mercury in the tube of the barometer, he employs the formula given by M. Biot, $(P) = P - \frac{PT}{5412}$, in which (P) represents the corrected height of the barometric column, P the observed height, T the temperature indicated by the thermometer attached to the barometer ; and the constant quantity $\frac{1}{5}\frac{1}{4}\frac{1}{1}\frac{1}{2}$ represents the true dilatation of the mercury for each degree, between the two limits of melting ice and boiling water. The value obtained by applying this correction shows the atmospheric pressure at the temperaature of 32° Fahrenheit.

According to M. Ramond, the most suitable hour for barometrical observation is noon, " because the equilibrium of the atmosphere, altered by the morning winds, is ordinarily restored towards the middle of the day." It is not so easy to determine the other hours ; M. Wallenstein has concluded, however, to make four meteorological observations a day, viz. at 7, 9, noon, and 4 o'clock.

" Among the changes," says M. Wallenstein, " which the atmosphere experiences, that which is indicated by the *horary variations* of the barometer, is not the least curious ; nor has it been well illustrated, even to the present time, either by theory

or by observations. M. de Humboldt has, perhaps, considered this subject the most fully in his *Geographie des Plantes*. He there collects the remarks of Godin and La Condamine on the horary oscillations of the barometer under the equator. Godin, according to him, first pointed out this phenomenon. According to La Condamine, these variations take place under the equator at 9 in the forenoon and 3 in the afternoon ; but he does not say at which of these times the barometer is at its maximum or minimum. M. de Humboldt concludes from his own observations, that along the shore of the South Sea, in the plain of the river Amazons, and in places of an elevation of 4000 metres, the times of these variations are constant ; that they are independent of the changes of temperature and of the seasons, and that the barometer is at its maximum at 9, A. M. that it does not vary its height, but slightly, till 12 ; but after noon it falls rapidly till 4 or $4\frac{1}{2}$, when it is at its minimum ; from this time it rises till 11 at night, when it is very nearly as high as it was at 9 the forenoon preceding ; it then falls anew till $4\frac{1}{2}$ o'clock in the morning, when it is almost as low as it was the preceding afternoon at 4 ; finally it rises again till it reaches its maximum at 9

"In [illegible] work, entitled *Experiences sur l'Initation des Fibres Mu[illegible]s et Nerveurses*, which is only in part translated into French, and of which the publication preceded by many years that which has just been cited, M. de Humboldt has considered atmospheric variations in their relation to physiology and pathology. 'If there constantly exist *the same difference* between the barometric pressure of two places, if for example in one of these places it was 28 inches, and in the other 24, organized bodies would probably be affected by the unequal density of the air, after the same manner, according to the law of habitual irritation. The rise and fall of the barometer *in the same place*, has a different effect. I have no doubt but that the countries where the variations of the barometer are the least considerable, are also the least healthful.' The salubrity of Peru, Chili, and mountainous regions generally, are cited in support of this opinion. 'I am inclined to believe,' says the same author, in another part of the same work, 'that the nature or the composition of the atmosphere, is less variable under the tropics than in the temperate regions of the globe. All meteorological phenomena seem there to follow a certain law, from which they very rarely vary. The diurnal flux and reflux of heat, of light, of electricity, and of the magnetic fluid are there more equal in their march, at least, so far as the few observations yet made, allow us

to judge. Even the barometer, the variations of which, so complicated in our climate, embarrass naturalists so much, rises and falls regularly, under the tropics, four times every twenty-four hours. Ought we to attribute to this regularity in the constitution of the atmosphere, the obstinate character of epidemic diseases in the climate of palms? In this climate when a disease fixes on the vital functions, it makes progress without being arrested by any external cause. The changes of the surrounding fluids are not sufficiently great to check decomposition. It is only at the arrival of a new season, that the power of external exciting causes triumphs over the morbid developement that has taken place in the fibre. In temperate climates, on the contrary, variations in the principle constituants of the atmosphere, are so frequent and so considerable, that they act powerfully on the vital functions. A change in the elasticity of the air, in the electric charge, in the quantity of oxygen, and in the humidity, modifies the vital functions, and the progress of the epidemic is, in part, checked by this continual modification of the external elements, or the disease shortly takes another form. Observing physicians have remarked, that in the temperate zone, places where the temperature is the most variable are the least exposed to epidemic disorders.'

"In this work, published in 1797, M. de Humboldt states the observations of Mr Moseley relative to the horary oscillations of the barometer under the tropics, as well as those of Mr Balfour made in Bengal. According to the *Tableau Physique des Regions Equinoxiales*, printed in 1805, these observations do not agree with those M. de Humboldt has himself made under the tropics.

"In Germany, notwithstanding the numerous perturbations which derange the march of the barometer, we find a certain regularity in the fall of the column of mercury, which takes place at two o'clock P. M., and in its elevation, which takes place at 8 o'clock in the evening.

"M. Cotte has deduced from a great number of observations made in Europe, that the barometer is at its minimum of elevation two hours after noon, and, consequently two hours sooner than under the equator. In our temperate climates the horary variations of the weight of the air, are perhaps concealed under a multitude of local causes, which make the barometer rise and fall irregularly. But with M. Van Swinden, I am far from doubting that the mean terms deduced from many thousand observations made from hour to hour, prove the barometer even in our climate, to rise and fall at certain regular intervals.

"M. Biot states the results to which a long series of observations conducted the celebrated naturalist of the Pyrenees, as regards the subject we are considering. M. Ramond has discovered that in France the barometer attains its maximum elevation about 9 o'clock A. M. after which it descends till about 4 o'clock P. M., when it is at its minimum; from this time it rises till near 11 P. M., when it reaches its maximum again; after which it commences a downward motion till 4 A. M., and thence it begins to return to the state first mentioned.

"The astronomer embarked on board the Russian vessels that in 1820 penetrated into high southern latitudes, observed during the navigation between the tropics, 'that the barometer experienced regularly, periodical and daily changes in such a manner that it attained its greatest elevation at 9 in the morning and 9 at night, and fell to the lowest point at 3 in the morning and 3 in the afternoon.'

"I have been for a long time interested in the horary oscillations of the barometer; indeed I think this subject merits the more attention, when we consider how little it has yet attracted the notice of scientific travellers. The same is the case with the diurnal variations of the magnetic needle, which few naturalists since Van Swinden have studied with attention and perseverance equal to his. Perhaps good observations on the periodical variations of the magnetic needle, of electricity, and of the barometer, collected by such men as La Condamine, Mutis, Humboldt, and Buch, may lead to the discovery of some connexion between these phenomena, or of the means of explaining them one by another.

"At least we have no reason to consider the time we occupy with these observations thrown away, while we know that the subject has engaged the attention of the philosophers we have just named, that a connexion has been remarked by one of them (M. Mutis) between the horary variations of the barometric column, and the conjunctions and oppositions of the moon, and that La Place has undertaken to calculate the influence that the sun and moon may have upon the atmosphere. We have need however, of all the authority of these great names to assist us in bearing up against the disfavour, and almost the ridicule which has been cast on this part of natural science by an astronomer, (M. Bode) renowned for his long and useful labours."

Meteorological Observations for November 1825, *made at Washington, D. C. Lat.* 38° 58′ *Long.* 77° 2′ *by J. De Wallenstein. Esq.*

Date.	Hours.	Thermom. Fahrenheit. °	Thermom. Average.	Hygrometer. °	Barometer. Apparent. ′	Barometer. Therm. Barom.	Barometer. Correction for the Temperature.	Barometer. Corrected.	Barometer. Inches.	Barometer. Average.	Observations and Appearance of the Heavens.	Winds.
						cent						
	7	39.2		92.0	0.7512	12.0	— 0.00168	0.7500	29.528		clear and calm.	NW.
1	12	61.7	55.1	68.0	0.7510	18.0	0.00252	0.7490	29.489	29.461	light wind.	South
	4	64.4		63.0	0.7480	18.5	0.00259	0.7469	24.367		cloudy, id.	South
	8	45.5		82.9	0.7552	13 0	0.00182	0.7539	29 682		fine weather, calm.	South
2	12	57.0	50.2	„	0.7567	16.0	0.00224	0.7550	29.725	29.727	clear strong wind.	West
	4	58.0		„	0.7582	17.0	0.00238	0.7563	29.776		id. id.	id.
	8	36.5		85.0	0.7662	9.0	0.00126	0.7655	30.119		fine, calm.	NNW.
3	12	53.6	48.8	70.0	0.7678	13.0	0.00182	0.7665	30.178	30.141	id. id.	South
	4	56.3		64.0	0.7672	18.0	0.00255	0.7652	30 127		id. id.	id.
	8	36.3		89.0	0.7697	10.0	0.00160	0.7686	30.261		fine, calm.	SW. b. S.
4	12	56.8	49.9	75 0	0.7705	13 0	0.00182	0.7692	30.294	30.277	id. id.	South
	4	56.6		71.0	0.7701	16.0	0.00224	0.7690	30.276		id. id.	South
	8	39.2		92.0	0.7719	11.0	0.00154	0 7709	30.351		hazy, calm.	NNW.
5	12	61.3	54.6	88.0	0.772$_0$	16 0	0.00224	0 7703	30.328	30.319	clear, id.	South
	4	63.5		75.0	0.7711	18.0	0.00252	0.7691	30.280		id. id.	SSE.

	8	43.3		84.0	0.7697	13.0	—	0.00182	0.7684	30.253		light fog, calm.	SSE.
6	12	71.6	61.8	70.0	0.7687	19.0		0.00266	0.7666	30.182	30.188	clear, id.	S. b. W.
	4	70.5		68.0	0.7675	19.6		0.00274	0.7653	30 131		id. id.	South
	8	50.7		90.0	0.7659	15.5		0.00217	0 7652	30 127		light fog.	NW.b.N.
7	12	68.0	59.2	66.0	0.7660	19.0		0.00266	0.7629	30.036	30.053	fine, calm.	South E.
	4	59.0		60.0	0.7640	19.0		0.00266	0.7619	29.996		id. id.	N. b. E.
	8	42.4		86.0	0 7619	12.0		0.00168	0.7607	29.950		light fog, calm.	NW. b.N.
8	12	66.2	57.5	64.0	0.7602	12.5		0.00175	0.7590	29.882	29.874	clear, calm.	South
	4	63.9		65.0	0.7587	18.0		0.00252	0.7667	29.792		cloudy.	South
	8	46.9		,,	0.7595	13.0		0.00182	0.7582	29.851		overcast, strong wind.	N. b. W.
9	12	42·6	45.6	100.0	0.7590	15.5		0.00217	0.7573	29.816	29.834	rain, id.	North
	4	47.3		,,	0.7592	14.0		0 00196	0.7578	29.835		cloudy, id.	North
	8	39.2		90.0	0.7609	11.0		0.00154	0 7599	29.917		fine weather, calm,	NNW.
10	12	50.4	46.5	66.0	0.7610	11.7		0.00164	0.7600	29.922	29.915	id. id.	W. b. N.
	4	50.0		64.0	0.7610	14.0		0.00196	0 7586	29.906		id. id.	N. b. W.
	8	34.2		98.0	0.7597	8.0		0.00112	0.7591	29.881		cloudy, calm	SW. b.W.
11	12	53.2	47.0	74.0	0.7590	13.0		0.00182	0.7577	29.832	29.850	id. id.	South
	4	53.6		,,	0.7595	14.0		0.00196	0.7580	29.843		id. id.	North
	8	40.6		70.0	0.7656	4.0		0.00056	0.7655	30.139		fair, calm.	West
12	12	49.1	47.1	54.0	0.7657	15.0		0.00210	0.7641	30.083	30.103	id. light wind.	W. b. N.
	4	51.8		48.0	0.7657	14.0		0.00196	0.7142	30.087		id. calm.	North
	8	45.1		76.0	0.7657	9.0		0.00126	0.7650	30.119		fair, calm	South
13	12	55.8	53.1	60.0	0.7632	12.5		0.00175	0·76≥0	30.001	30.015	id. brisk wind.	South
	1	55.4		65.0	0.7616	14.0		0.00196	0.7601	29.926		id. id.	SSE.
	8	55.0		100.0	0.7547	16.0		0.00224	0.7530	29.646		rain, strong wind.	North
14	12	52.7	52.5	,,	0.7567	16.0		0.00224	0.7550	29.725	29.711	cloudy, wind abating.	NW.
	4	50.0		,,	0.7580	15.0		0.00210	0.8567	29.480		fair. calm.	"
	8	42.2		95.0	0.7602	11.6		0.00164	0.7592	29.890		cloudy, light wind.	South
15	12	55.0	49.8	60.0	0.7605	14.5		0.00203	0.7590	29.882	29.883	id. id.	id.
	4	52.3		75.0	0.7605	15.0		0.00210	0.7589	29.878		id. id.	SE. b. S.

Date.	Hours.	Thermom. Fahrenheit. °	Thermom. Average.	Hygrometer. °	Barometer. Apparent. '	Thermom. Barom. cent	Correction for the Temperature.	Barometer. Corrected.	Barometer. Inches.	Barometer. Average.	Observations and Appearance of the Heavens.	Winds.
	8	42.3		80.0	0.7597	9.8	— 0.00137	0.7588	29.874		clear, calm.	North
16	12	46.8	44.5	68.0	0.7600	12.8	0.00179	0.7587	29.870	29.875	id. light wind.	West
	4	44,6		72.0	0.7600	11.0	0.00154	0.7590	29 882		id. id.	NW.
	8	40.8		90 0	0.7559	11.0	0.00154	0.7549	29.721		cloudy calm.	South
17	12	46.2	43.3	92.0	0.7522	13.0	0.00182	0.7509	29.564	29.602	id. id.	S. b. W.
	4	42.8		92.0	0.7509	12.0	0.00168	0.7497	29.517		id. id.	"
	8	30.5		„	0.7557	7.7	0 00098	0.7552	29.733		clear, violent wind.	NW.
18	12	„	31 2	„	0.7565	6.8	0.00095	0.7561	29.768	29.788	„ „	NW.
	4	32.0		„	0.7590	7.0	0.00098	0.7585	29.863		„ „	NW.
	9	30.2		68.0	0.7650	3.5	0.00049	0.7656	30.119		fair, calm.	NNE.
19	12	38.7	35.4	60.0	0.7630	8.0	0.00112	0.7644	30.095	30.083	id. id..	SW. b.W.
	4	37.4		65.0	0.7636	8.5	0.00119	0.7629	30.036		id. id.	South.
	8	39.2		75.0	0.7605	6.5	0.00091	0.7601	29.926		cloudy, light wind.	S. b. E.
20	1	51.8	47.0	57.0	0.7600	10.0	0.00140	0.7591	29.886	29,895	fair, calm.	W. b. N.
	4	50.0		62.0	0.7600	12.0	0.00168	0.7588	29.874		id. id.	SSE.
	8	37.0		97.0	0.7629	7.0	0 00098	0.7624	30.017		hazy, calm.	North
21	12	53.6	45.2	65.0	0.7631	11.5	0.00161	0.7620	30.001	29.997	cloudy, id.	SE.
	4	45.1		88.0	0.7625	12.0	0.00168	0.7613	29.773		id. id.	South
	8	35.4		94.0	0.7624	9.0	0.00126	0.7617	29.989		foggy, calm.	NW.
22	12	49.3	45.8	79.0	0.7625	10.6	0.00148	0.7615	29.981	29.990	id. brisk wind.	NNW.
	4	52.7		00.0	0.7635	14.7	0.00205	0.7629	30.001		light fog, calm.	NW.

	8	25.3	}		98.0	0.7674	4.8	—	0.00067	0.7672	30.205	}	fair, calm.	NNW.
23	12	41.0	}	37.2	81.0	0.7667	10 0		0.00140	0.7658	30.150	} 30.137	id. id.	South
	4	45.5	}		78.0	0.7645	10.0		0.00140	0.7636	30.064	}	id. id.	S. b. E.
	9	39.2	}		100.0	0.7600	8.5		0.00119	0.7597	29.908	}	cloudy, calm.	SW.
24	12	46 4	}	45.6	85 0	0.7596	11.0		0 00154	0.7586	29.867	} 29.878	id. id.	NW.
	4	51.3	}		64.0	0.7596	12.0		0.00168	0.7584	29.859	}	id. brisk wind.	NW.
	9	41.0	}		94.0	0.7610	8.0		0.00126	0.7603	29.934	}	fair and calm.	NW.
25	"	"	}	44.1	"	"	"		"	"	"	} 29.916	"	"
	4	47 3	}		69.0	0.7610	15.0		0.00210	0.7594	29.898	}	id. id.	id.
	9	32.9	}		99.0	0.7628	7.6		0.00106	0·7623	30 013	}	fair, calm,	NW.
26	12	48.2	}	43.2	75.0	0.7630	10.0		0.00140	0.7621	30 005	} 30.003	id. id.	id.
	4	48.7	}		73.0	0.7630	12.0		0.00168	0.7618	29.992	}	id. id.	NE.
	8	25.0	}		98.0	0.7633	5.8		0.00081	0.7630	30.040	}	fair, calm, white frost.	NNW.
27	12	49.0	}	41.3	85.0	0 7632	9.5		0.00133	0.7624	30.017	} 30.007	fair, calm.	NNW.
	4	50.0	}		80.0	0.7630	17.5		0.00243	0.7611	29.965	}	id. id.	East
	8	31.0	}		92.0	0.7634	10.0		0.00140	0.7625	30.021	}	fair, calm,	NW.
28	12	47.0	}	41.6	80.0	0.7632	12.5		0.00175	0.7620	30 001	} 30.010	id. id.	NW.
	4	47.0	}		71.0	0.7632	10 5		0.00147	0.7622	30.009	}	id. id.	East
	8	26.0	}		90.0	0.7630	17.5		0.00243	0.7611	29.965	}	fair, calm.	NE.
29	12	49.0	}	41.8	76.0	0.7632	11.5		0 00161	0.7621	30.005	} 29.970	id. id.	SE.
	4	50 5	}		74.0	0.7626	18.0		0.00252	0.7606	29.946	}	id id.	SE.
	8	40 0	}		93.0	0.7611	9.0		0.00126	0.7603	29.934	}	cloudy, calm.	NE.
30	12	43.5	}	42.2	92.0	0.7610	13.5		0.00189	0.7596	29.906	} 29.901	id. id.	North
	4	43.0	}		94.0	0.7603	14.0		0.00196	0.7588	29.874	}	id. id.	W. b. N.

THERMOMETER.

Average Heat,	48°,2
Maximum the 6th at 12 o'clock,	71, 6
Minimum the 27th, at 9 A. M.	25, 0
Range,	46,6

BAROMETER.

	Inches.
Average Height,	29,944
Maximum the 5th, at 12 o'clock,	30,328
Minimum the 1st at 4 P. M.	29,367
Range,	0,961

HYGROMETER.

Average dampness of the air,	76°,7
Greatest do. (rain),	100, 0
(The 12th at 12,) least dampn.	54, 0
Difference,	46, 0
Number of rainy days,	2
Total rain,	0,24 inch.

Abstract of Meteorological Observations for the year 1825, *taken in Boston, in Lat.* 42° 21′ 58″ *N., Long.* 71° 3′ 45″ *W. Communicated by R. T. Paine, Esq.*

	Barometer.		Thermometer.	
January.				
Mean altitude,	30,006			129,
Highest, 22d,	30,56		12th,	49
Lowest, 2d	28,99		8th,	8
Range,	1,57			41
February.				
Mean altitude,	30,023			32,0
Highest, 8th	30,58		24th,	51
Lowest, 2d,	29,30		4th,	7
Range,	1,28			44
March.				
Mean altitude,	29,918			40,8
Highest, 16th,	30,38		20th,	65
Lowest, 31st,	29,34		23d,	28
Range,	1,04			37
April.				
Mean altitude,	29,960			50,1
Highest, 2d,	30,44		16th,	80,
Lowest 11th,	29,48		2d,	33
Range	,96			47
May.				
Mean altitude,	30,025			57,6
Highest, 15th,	30,36		31st,	81
Lowest, 5th,	29,58		3d,	34
Range,	,78			47
June.				
Mean altitude,	29,884			70,1
Highest, 30th,	30,11		11th,	96
Lowest, 25th,	29,42		4th.	53
Range,	,69			43
July.				
Mean altitude,	29,939			76,4
Highest, 9th,	30,17		11th,	100
Lowest, 24th,	29,74		28th,	60
Range,	,43			40
August.				
Mean altitude,	29,978			69,5
Highest, 19th,	30.32		6th,	89
Lowest, 9th,	29,62		27th,	54
Range,	,70			35
September.				
Mean altitude,	29,993			61,1
Highest, 12th,	30,30		15th,	79
Lowest, 24th,	29,46		25th,	45
Range,	,84			34

			OCTOBER.				
Mean altitude,			30,004				54,4
Highest,	22d,		30,49		7th,		88
Lowest,	12th,		29,56		22d,		30
Range,			,93				58
			NOVEMBER.				
Mean altitude.			30,147				41,6
Highest,	4th,		30,70		6th,		65
Lowest,	18th,		29,46		19th,		17
Range,			1,24				48
			DECEMBER.				
Mean altitude,			29,965				33,2
Highest,	23d,		30,58		17th,		58
Lowest,	31st,		29,41		13th,		—5
Range,			1,17				63
Mean of 1st 6 months,			29,969				46,70
„ „ last 6 do.			30,004				56,10
„ of the year,			29,987				51,44
Highest. Nov. 4th,			30,70		July 11th,		100
Lowest, Jan 2d,			28,99		Dec. 13th,		—5
Range during the year,			1,71				105

The mean temperatures of all the months in the last year, except August and September, are from two to five degrees greater than those deduced from observations of the last thirty years; and the mean of the whole year is 2,84 degrees greater.

Three observations were made in each day, at sunrise, 3 P. M., in summer, 2½ P. M. when the sun was on the equator, and 2 P. M. in the winter, and at 10 in the evening. The mercury in the cistern of the barometer is fourteen feet above the mean level of the sea.

General Intelligence.

On the Thermometrical State of the Terrestrial Globe.—M. Arago, in an article in the "Annales de Physique," discusses the question of the temperature of the globe at its surface and arrives at this conclusion, that in Europe in general, and in particular in France, the winters some centuries back, have been as cold as at present. He grounds his opinion upon the fact of the freezing of the rivers and seas at a great number of periods even of very remote date. The author then gives a table of the extreme temperatures observed at Paris, from which there results that in the second half of the last century, the greatest cold (—23,5° cent.) took place in the

25th January, 1795, and the greatest heat (38,4°) on the 8th July, 1793. He then gives the temperatures observed during the expeditions of Captains Parry and Franklin, and the dates of the natural congelation of mercury, together with the tables of the maximum temperatures of the atmosphere observed on the open sea at its surface. From these observations together, M. Arago draws the following conclusions: 1st, In no part of the earth on land, and in no season, will a thermometer, raised from 2 to 3 metres above the ground and protected from all reverberation, attain the 46 centigrade degree; 2dly, In the open sea, the temperature of the air, whatever be the place or season, never attains the 31st centigrade degree; 3dly, The greatest degree of cold which has ever been observed on our globe with a thermometer suspended in the air, is 50 centigrade degrees below zero; 4thly, The temperature of the water of the sea in no latitude, and in no season, rises above 30 centigrade degrees. [*Ann. de Phys. et de Chim.*]

Hawkins's Mode of preparing Emery.—Mr Hawkins, finding that the emery sold in the shops was totally inefficient for the purpose he had in view, namely, grinding two flat surfaces of hard cast steel accurately, thought of applying a process he had seen for washing over *diamond dust*, to emery; and to be certain that his emery was of good quality, he purchased of an emery-maker a quantity of those small lumps, or grains, which had longest withstood the action of the cast-iron runners and bed, and thus ensured the hardness of the emery; these pieces were reduced to powder in a cast-iron mortar, and then separated into different portions by sieves.

He then washed over the finest emery thus obtained, using oil instead of water, as in the usual process, the former holding it in suspension for a much longer time; and in this way obtained a series of emery which had floated one, five, ten, fifteen, twenty, forty, and eighty minutes, amongst which he fonnd every variety necessary for his purpose; and keeping them in boxes, which were numbered according to the minutes they had floated, he could at any time prepare more of any one kind. In this way Mr Hawkins readily attained his object, and ultimately by selecting those grains of emery which resisted longest the action of the pestle and mortar, he obtained some so hard, as to be capable of cutting a ruby, when employed instead of diamond-dust.

Mr Gill by grinding Greek emery-stone between two flat and hard steel surfaces, and washing off the lighter portions

in oil, found that those which subsided in half a minute, when examined by a microscope, had entirely resisted the friction, and were perfectly crystallized sapphires. [*Tech. Rep.*]

On the Umbilicus of Marsupial Animals.—It has been generally thought that Marsupial animals are destitute of any umbilicus, and are only attached to the mother by means of the mouth.

Geoffroy St Hilaire has lately discovered in some specimens of the fœti of *Didelphis Virginiana* preserved in spirits, which had been taken from the mother by Dr Barton very soon after their introduction into the pouch, evident vestiges of placental organization, and of an umbilicus. They were only five lines long, and already formed; in the two males which he examined, the umbilicus was large for the size of the animal, as it was also in the female, and very distinct from the entrance of the pouch.

Mr Geoffroy observes, that the series of transformations common to all mammalia are *ovulum*, *embryo*, and fœtus. These three stages of genital products require three distinct situations, which in the other mammalia are found in the sexual canal, but in the marsupialia they are very differently distributed, though in an equally continuous series. The ovulum and the embryo are formed and developed in the sexual canal and the fœtus out of it. The womb is the third station in the mammalia, where the common fœtus is incubated and nourished; and the *marsupium* or nursing pouch is for the same purpose in the marsupial animals. The difference, therefore, consists only in the name of the last part. [*Ann. Sci. Nat. and Zool. Jour.* 1. 403.]

Capillary Attraction.—M. Gillerson says, "If a capillary tube be introduced into mercury, the metal will remain in the tube below the exterior surface. If then the tube be carefully raised, without taking it out of the mercury, the surface of the mercury in the tube may be raised to the level of that without. Operating very carefully, it may even be raised still higher; its surface will then become concave, the nature of the curve apparently approaching that of the catenarian curve, which I believe also to be that of liquids which in capillary tubes are raised above the level of the external surface. If then the tube be depressed a little, the convex surface may be again given to the mercury in the tube, without its level being depressed below that of the external portion. It appears to me therefore, that the surface of liquids in capil-

lary tubes is an accessary circumstance, and has no direct influence on the elevation or depression of the liquid." [*Bib. Univ.* XXII. 209.]

On Combinations of Carbon and Iron, Pig Iron, &c.—A long experimental memoir by M. K.rsten, on the combination of iron and carbon, is contained in the Annales des Mines, IX. 657. We have not time at present to analyze it, but give the results as drawn up by the author, and appended to the memoir.

1. White cast iron and tempered steel contains the carbon combined with the whole mass of iron. 2. Lamellated white cast iron presents a perfect combination of iron with carbon; it always contains more carbon than the grey cast iron. 3. Iron and steel, not tempered, contains the carbon in the state of carburet. 4. Cold grey cast iron contains the larger part of its carbon, in the state of graphite and of mixture: This graphite contains no iron, but constitutes the carbon in all its purity. 5. The rest of the carbon contained in the grey cast iron, may be found either combined with the whole mass, or forming a definite carburet which is afterwards dissolved in the metal, as is the case with soft iron, or steel. 6. All the varieties of carburetted iron, considered in the liquid state, contain the carbon dissolved in the mass of metal in indefinite proportions. 7. Finally. The graphite separates from the metal at the moment of congelation, and if there be other carburets of iron, they separate at a later period.

Remarking on the means generally proposed for the separation or estimation of the carbon in carburetted iron, M. Karsten finds grounds of objection to them all. That proposed by M. Vauquelin, namely, the use of the sulphurous acid, he states to be uncertain and inacurate from the formation of sulphuret of iron. [*Jour. of Sci. Lit. and Arts.*]

Antiquity of Trees.—In "Major Rooke's Sketch of the Forest of Sherwood," it is stated that in cutting down some timber in Berkland and Bilhaugh, *letters* have been found cut or stamped in the body of the trees, denoting the king's reign in which they were marked. It seems that the bark was cut off, and the letters cut in, after which the next year's wood grew over it, but without adhering where the bark had been cut. The ciphers are of James I., of William and Mary, and one of King John! One of those with James's cipher was about one foot within the tree, and one foot from the centre: it was cut down in 1786. The tree must have been two feet

in diameter, or two yards in circumference, when the mark was cut. A tree of this size is generally estimated at 120 years growth, which number substracted from the middle year of James's reign would make 1492 the date of the planting of the tree. The tree with William and Mary had the mark about nine inches within the tree, and three feet three inches from the centre; cut down also in 1786. The mark of John was eighteen inches within the tree, and something more than a foot from the centre: it was cut down in 1791; but the middle year of John's reign was 1207, from which if we substract 120, the number of years requisite for a tree two feet diameter to arrive at that growth, it will make the date of its planting 1085, or about twenty years after the conquest. The tree therefore, when cut down in 1791, must have been 706 years old; a fact scarcely credible; for it appears from the trees whose marks are better authenticated, that those exactly of the same size, when marked, had increased twelve inches in diameter in 173 years, whilst this tree had increased no more than eighteen inches in 534 years. Major Rooke says that several trees with this mark had been cut down, so that deception or mistake is scarcely possible. [*N. M. Mag.*]

Light of Haloes.—M. Arago, from observations made on the 11th April, 1825, with the instrument which he has invented for the examination of polarized light, has discovered that the light of haloes (luminous circles which sometimes appear round the sun, and whose apparent diameters are $22\frac{1}{2}^{\circ}$ and 45°) is not a reflected, but a refracted light; a result which gives much probability to the explanation of the phenomenon proposed by Mariotte. This philosopher supposed that the solar ray is refracted in its passage through the drops of water frozen and suspended in the atmosphere. M. Arago is of opinion, that the observation of haloes might lead to the discovery of the true law of the decrease of temperature in proportion as we rise from the earth's surface, a law which hitherto has had no other foundation than a single ærostatic ascension of Gay-Lussac.—[*Bullet. Univ. May*, 1825.]

Sir H. Davy's method of protecting the Copper Sheathing of Ships.—Several accounts have been published in this Journal of the labours of Sir H. Davy in attempting to protect the copper sheathing of ships from corrosion. An unusual excitement has prevailed in England, upon this subject, for more than a year past; an excitement which may be attrib-

uted more perhaps to the high character of Sir H. Davy, than to the importance of the experiments in which he has been engaged. It appears by late accounts in English publications, that ships protected by iron, have been found so generally *foul*, both with the fleshy and hard shell species of *barnacles* after a voyage, that the admiralty have given orders to suspend the use of the protectors on all national sea going ships; but they are to be continued on vessels in ordinary, receiving ships, hulks, &c.

A correspondent of this Journal has favoured us with a short paper on this subject; part of which, being a recapitulation of preceding accounts, it is not necessary to publish. The following are extracts from this paper:—

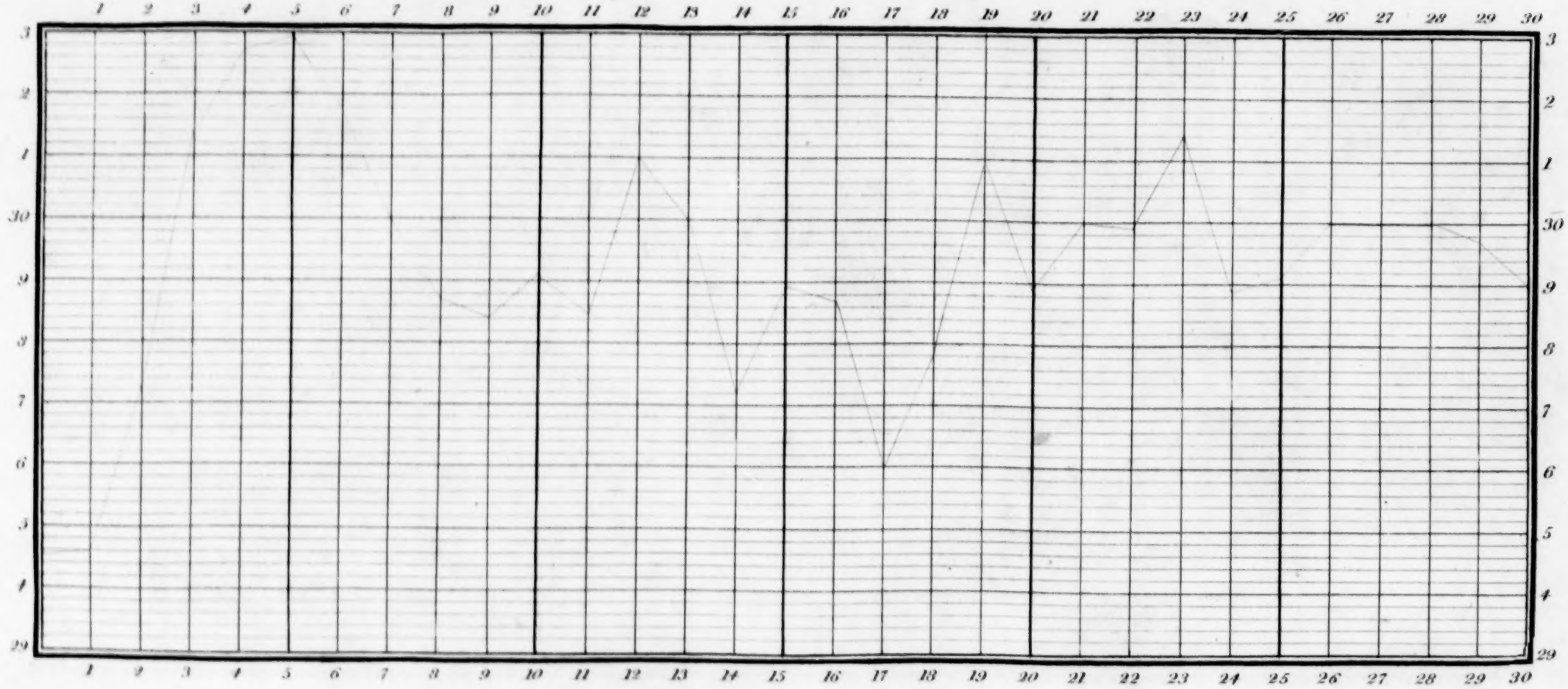
"If I am right in the conjecture, the copper sheathing, as generally used, does not become foul, because the rust that readily wastes it in the sea, is so peculiarly corrosive and destructive, that nothing possessing animal or vegetable life will adhere to it. The difficulty is, that the new method, while it saves the copper, prevents the oxidation, which keeps it clear of these destructions, and unless some remedy shall be discovered, the method must undoubtedly be given up.

"This idea has occurred to me, that the plates of iron, zinc or tin, may be secured to the vessel in such a manner, as to be capable of being occasionally removed at sea, so as to allow a partial oxidation of the copper sufficient to detach any of the vegetable or animal substances that may be collecting, and then replaced. They might no doubt be fixed so as to be connected with the copper, and be continually wet, which would give them the operation desired, and still be within reach. Sir H. Davy found that a small mass of tin, even when communicating only by a wire with a large plate of copper, entirely preserved it. Experiments might easily be made to ascertain whether weeds and barnacles would not fall off from copper, when oxidation was suffered to take place; and also for how long a time it would be necessary the metal should be subjected to the action of sea water, in order to effect such oxidation to a sufficient extent."

If no experiments founded on views like these, have been made, we hope that they will be instituted upon these suggestions, as it would certainly appear that a considerable reduction of the oxidation, by means of the protectors, is not incompatible with a clean state of the copper.

Table showing the range of the Barometer at Washington. Nov.r 1825.

Eng.d for Mr. Wallenstein's paper.



Art. XXIV.—*Biographical Account of M. le Comte Lagrange.* By M. le Chevalier Delambre. [*Ann. Philos.*]

(Concluded from p. 114.)

M. Lagrange took possession of his situation on the 6th of November, 1766. He was well received by the king; but soon perceived that the Germans do not like to see foreigners occupy situations in their country. He applied to the study of their language. He devoted himself entirely to mathematics, and did not find himself in the way of any person, because he demanded nothing; and he soon obliged the Germans to give him their esteem. "The king," said he himself, "treated me well, I thought that he preferred me to Euler, who was something of a devotee, while I took no part in the disputes about worship; and did not contradict the opinion of any one." This prudent reserve, if it deprived him of the advantage of an honourable familiarity, which would have been attended with some inconveniences, left him the whole of his time for mathematical labours, which hitherto had brought him nothing but compliments the most flattering and the most unanimous. This concert of praises was only once interrupted during the whole of his life.

A French mathematician, who to much sagacity united a still greater degree of selfishness, and scarcely gave himself the trouble to study the works of others, accused M. Lagrange of *having gone astray in the new route that he had traced, from not having well understood the theory of it.* He reproached him with *having deceived himself in his assertions and calculations.* Lagrange in reply expresses some astonishment at these harsh expressions, to which he was so little accustomed. He expected at least to have seen them founded upon some reasons either good or bad; but he discovered nothing of the kind. He shows that the solution proposed by Fontaine was incomplete and illusory in certain respects. Fontaine had boasted that he had taught mathematicians the c[illegible]ich render possible the integration of differential eq[illegible] three variables. Lagrange showed him by several [illegible]hat these conditions were known to mathematicians [illegible]e Fontaine was capable of teaching them. He does [illegible]y that Fontaine discovered these theorems himself, "[illegible] I am persuaded," says he, "that he was as capable [illegible]g them as any person whatever."

It was w[illegible] s delicacy and moderation that he answered the aggressor. [illegible]rcet, in his *éloge* of Fontaine, is obliged to avow that, on th[illegible]casion, his friend deviated from that politeness

which ought never to be dispensed with, but which perhaps he thought less necessary with illustrious adversaries, whose glory did not stand in need of these little delicacies. Every one can estimate the value of that apology, especially when applied to a man who, by his own acknowledgment, studied the vanity of others that he might wound it upon occasion. We must at least acknowledge that he, who saw himself attacked in that manner when he was in the right, and who knew how to maintain politeness with an adversary who had himself dispensed with it, acquired a double advantage over him, besides victoriously repelling his imprudent attack.

It will not be expected that we should follow M. Lagrange in the important researches with which he filled the Berlin Memoirs; and even some volumes of the Memoirs of the Turin Academy, which was indebted to him for its existence. All the space that can be devoted to this biographical account would not be sufficient even to give an imperfect idea of the immense series of his labours, which have given so much value to the Memoirs of the Berlin Academy, while it had the inestimable advantage of being directed by M. Lagrange. Some of these Memoirs are of such extent and importance that they might pass for a great separate work, yet they constitute a part only of what these twenty years enabled him to produce. He had composed his Mecanique Analytique, but he wanted to have it printed at Paris, where he expected that his formulas would be given with more care and fidelity. On the other hand, it was running too great a risk to intrust the manuscript into the hands of a traveller, who might not be aware of the whole of its value. M. Lagrange made a copy of it, which M. Duchatelet undertook to deliver to the Abbé Marie, with whom he was intimately connected. Marie fulfilled with honour the confidence placed in him. His first care was to find a bookseller who would undertake to publish it; and, what it will be difficult to believe at this time, he could not find one. The newer the methods in it were, and the more sublime the theory, the fewer readers would be found capable of appreciating it; hence, without entertaining any doubts of the merit of the work, the booksellers were excusable in hesitating to print a book, the sale of which would probably be confined to a small number of mathematicians disseminated through Europe. Desain, who was the most enterprizing of all those to whom application was made, would not undertake to publish it, till Marie entered into a formal engagement to take all the copies of the edition which were not sold by a given time. To this first service Marie added another, of which M. Lagrange was not less sensible; he procured him

an editor worthy of superintending the publication of such a work. M. Legendre devoted the whole of his time to the troublesome task of correcting the press, and was repaid by the sentiment of veneration for the author with which he was penetrated ; and by the thanks which he received from him in a letter which I have had in my possession, and which M. Lagrange had filled with expressions of his esteem and his gratitude.

The book was not yet published when the author came to settle in Paris. Several causes determined him to take this step ; but we must not give credit to all that have been stated. The death of Frederic had occasioned great changes in Prussia, and still greater were to be apprehended. Philosophers were no longer so much respected as formerly. It was natural for M. Lagrange again to feel that desire which had formerly conducted him to Paris. These causes, together with the publication of the Mecanique Analytique, were sufficient. It is not necessary to add other causes, which several publications that made their appearance in Germany, and particularly the anonymous historian of the court of Berlin, have noticed. We never, during a residence of twenty-five years in France, heard M. Lagrange prefer the slightest complaint against the minister, who is accused in that publication of having disgusted him by a treatment full of haughtiness and contempt, which out of respect for himself it was impossible for M. Lagrange to overlook. We might suspect that M. Lagrange had sufficient generosity to forget or pardon bad treatment, which he punished in the only way worthy of himself, by leaving the country where his merit was overlooked ; but when he was directly questioned on that subject by a Member of the Institute (M. Burckhartd) he only gave negative answers, and assigned no other motives than the misfortunes which it was thought were about to fall upon Prussia. M. de Hertzberg was dead, and M. le Lagrange, a senator and comte of the French empire, could have no interest in concealing the truth. Hence we must consider his own statement as affording the only true reasons.

The historian therefore, whom we have quoted, has been ill-informed. But the spirit of calumny and satire, which has so justly rendered his work suspected, ought not to prevent us from extracting from it the lines in which he explains, with that energy which is peculiarly his own, his opinion, which is that of all Europe, when he does justice to M. Lagrange.

" I think," says he, (Hist. Secrete de la Cour de Berlin, 1789, tome ii. page 173), " that there is at this moment an acquisition worthy of the king of France, the illustrious Lagrange, the great-

est mathematician who has appeared since Newton, and who in every point of view is the man that has the most astonished me ; —Lagrange, the wisest, and perhaps the only practical philosopher that ever existed, meritorious by his undisturbable wisdom, his manners, his conduct ; the object of the most tender respect of the small number of men with whom he associates ;—Lagrange is misunderstood ; every thing leads him to leave a country where nothing can excuse the crime of being a foreigner, and where in fact he is merely tolerated. Prince Cardito de Laffredo, Neapolitan minister at Copenhagen, offered him the most flattering conditions on the part of his sovereign. The Grand Duke, the King of Sardinia, invite him eagerly ; but all their proposals would be easily obliterated by ours. I am very eager to see this proposal made, because I consider it as noble, and because I tenderly love the man who is the object of it. I have induced M. Lagrange not to accept immediately the proposals made to him, and to wait till he receives ours."

The author whom we quote appears to fear the opposition of M. Breteuil ; but, according to M. Lagrange himself, it was the Abbé Marie who proposed it to M. Breteuil, who on all occasions anticipated the desires of the Academy of Sciences, presented the demand to Louis XVI, and induced him to agree to it.

The successor of Frederic, although he did not much interest himself in the sciences, made some difficulty in allowing a philosopher to depart whom his predecessor had invited, and whom he honoured with his particular esteem. After some delay, M. Lagrange obtained liberty to depart. It was stipulated that he should still give some memoirs to the Berlin Academy. The volumes of 1792, 1793, and 1803, show that he faithfully kept his promise.

It was in 1787 that M. Lagrange came to Paris to take his seat in the Academy of Sciences, of which he had been a foreign member for fifteen years. To give him the right of voting in all their deliberations, this title was changed into that of *veteran pensionary*. His new associates showed themselves happy and proud in possessing him. The Queen treated him with regard, and considered him as a German. He had been recommended to her from Vienna. He obtained a lodging in the Louvre, where he lived happy till the Revolution.

The satisfaction which he enjoyed did not show itself outwardly. Always affable and kind when interrogated, he himself spoke but little, and appeared absent and melancholy. Often in companies which must have been suitable to his taste, among the most distinguished men of all countries who met at the house of the illustrious Lavoisier, I have seen him dreaming as it were,

with his head against a window, where however nothing attracted his attention. He remained a stranger to what was passing around him. He acknowledged himself that his enthusiasm was gone, that he had lost his taste for mathematics. When informed that a mathematician was employed at such a task, " so much the better," he would say, " I had begun it, now it will be unnecessary for me to finish it." But he merely changed the object of his studies. Metaphysics, the history of human nature, that of different religions, the general theory of languages, medicine, botany, divided his leisure hours. When the conversation turned upon subjects with which it was supposed he was unacquainted, we were struck by an unexpected observation, a fine thought, a profound view, which excited long reflections. Surrounded by chemists who were reforming the theory and even the language of the science, he made himself acquainted with their discoveries, which gave to facts formerly isolated, that connexion which distinguishes the different parts of the mathematics. He undertook to make himself acquainted with this branch of knowledge, which formerly appeared to him so obscure, but which he found on trial *as easy as algebra*. People have been surprised at this comparison, and have thought that it could come from no one else than Lagrange. It appears to us as simple as just ; but it must be taken in its true sense. Algebra, which presents so many insoluble problems, so many difficulties against which Lagrange himself struggled in vain, could not in that sense appear to him an easy study. But he compares the new elements of chemistry with those of algebra. They constituted a body, they were intelligible, they offered more certainty, they resembled algebra, which in the part of it which is complete presents nothing difficult to conceive, no truth to which we may not arrive by the most palpable reasoning. The commencement of the science of chemistry appeared to him to offer the same advantages, perhaps with somewhat less stability and certainty ; but, like algebra, it has no doubt also its difficulties, its paradoxes, which will require, to explain them, much sagacity, reflection, and time. It has likewise its problems, which never will be resolved.

In this philosophic repose he continued till the revolution, without adding any thing to his mathematical discoveries, or even opening his Mecanique Analytique, which had been published for two years.

The revolution gave philosophers an opportunity of making a great and difficult innovation : the establishment of a system of weights and measures founded on nature, and perfectly analogous to our scale of numbers. Lagrange was one of the commission-

ers whom the Academy charged with that task. He was one of its keenest promoters. He wished to see the decimal system in all its purity. He was provoked at the complaisance of Borda, who got quarters of the *metre* made. He thought the objection of little importance which was drawn against the system from the small number of divisors that its base afforded. He regretted that it was not a prime number, as 11, which would have given the same denominator to all the fractions. This idea perhaps will be regarded as one of those exaggerations, which are hazarded by men of the best understandings, in the heat of dispute. But he mentioned the number 11 merely to get rid of the number 12, which more intrepid innovators would have wished to substitute in place of the number 10, that constitutes the base of the whole of our numeration.

When the Academy was suppressed, the commission charged with the establishment of the new system was retained for a time. Three months had scarcely elapsed when, in order to *purify* that commission, the names of Lavoisier, Borda, Laplace, Coulomb, Brisson, and Delambre, were struck out. Lagrange was retained. In quality of president, he informed me, in a long letter full of kindness, that I should receive official information of my removal. As soon as he saw me on my return to Paris, he expressed to me his regret at the dismission of so many associates. "I do not know," said he, "why they have retained me." But unless the suppression had been total, it could scarcely have extended to him. The more losses the commission had sustained, of the more importance was it not to deprive it of the consideration attached to the name of Lagrange. Besides, he was known to be wholly devoted to the sciences; he had no place either in the civil department or the administration. The moderation of his character had prevented him from expressing what he could not but think in secret: but I shall never forget the conversation which I had with him at that period. It was the day after the atrocious and absurd sentence, contrary to every thing like justice, had thrown all lovers of the sciences into mourning, by cutting off the most illustrious philosopher in Europe. "It has cost them but a moment," said he, "to cut off that head, and a hundred years perhaps will not be sufficient to produce another like it." Some months before we had had a similar conversation in the cabinet of Lavoisier, on account of the death of the unfortunate Bailly. We lamented together the dreadful consequences of the dangerous experiment which the French had attempted. All these chimerical projects of melioration appeared to him very equivocal proofs of the greatness of the human

mind. " If you wish to see it truly great," added he, " enter into the cabinet of Newton employed in decomposing light, or in explaining the system of the world."

Already for some time he had regretted not having listened to the advice of his friends, who at the commencement of our troubles had recommended him to seek an asylum, which it would have been so easy for him to find. As long as the revolution seemed only to threaten the pension which he enjoyed in France, he had neglected that consideration, out of curiosity to be upon the spot of one of those great convulsions which it is always more prudent to observe at a distance. " It was your own choice," said he several times to himself when he entrusted me with his regret. It was to no purpose that a special decree of the Constituent Assembly had insured the payment of his pension. The decree was of no value, because the depreciation of the paper currency was sufficient to render it illusory. He had been named member of the *Board of Consultation*, appointed to examine and reward useful inventions. He had been appointed one of the administrators of the Mint. This commission offered him few objects to fix his attention, and could in no degree remove his apprehensions. It was again proposed to draw him to Berlin, and to restore him to his former situation. He had agreed to the proposal. Herault de Sechelles, to whom he had applied for a passport, offered him, for the greater security, a mission to Prussia. Madame Lagrange would not consent to quit her country. This repugnance, which at that time he considered as a misfortune, was to him a source of fortune and of new glory.

The Normal School, of which he was named Professor, but which had only an ephemeral existence, scarcely gave him time to explain his ideas respecting the foundation of arithmetic and algebra, and their application to geometry.

The Polytechnic School, the result of a happier idea, had likewise a more durable success: and among the best effects which it produced, we may place that of having restored Lagrange to Analysis. It was there that he had an opportunity of developing those ideas, the germ of which was to be found in two memoirs that he had published in 1772, and the object of which was to explain the true metaphysics of the differential and integral calculus. To render these happy developements more easily understood, the professor associated himself with his pupils. It was then that he composed his Analytical Functions, and his Lectures on that Calculus, of which he published several editions. " Those who have it in their power to attend these interesting lectures," said publicly one of the professors (M. Lacroix), " have the

pleasure to see him create before the eyes of his audience almost the whole of his theory, and will carefully preserve several variations which the historian of the science will collect as examples of *the path followed in analysis by the genius of invention.*"

It was then likewise that he published his treatise on the numerical solution of equations, with notes on the several points of the theory of algebraic equations.

It is said that Archimedes, whose great reputation, at least with the historians, is founded upon the machines of all kinds, by means of which he retarded the taking of Syracuse, despised these mechanical inventions, on which he wrote nothing, and placed importance only in his works of pure theory. We may sometimes conceive that the great mathematicians of our age entertained the same sentiments with Archimedes. They consider a problem as solved when it presents no analytical difficulty, when nothing remains to be done but differentiations, substitutions, and reductions, operations which require merely patience, and a certain dexterity derived from practice. Satisfied with having removed all the real difficulties, they concern themselves perhaps too little with the embarrassments which they leave to the calculator, and with the long labour necessary in order to make use of their formula, even after it has been suitably reduced. M. Lagrange had more than once attempted to abridge the usual calculations.

The general resolution of algebraic equations is subject to difficulties which are considered as insurmountable ; but in practice every determinate problem brings us to an equation, all the coefficients of which are given in numbers. It would be sufficient therefore to have a sure method of finding all the roots of such an equation, which is called *numerical*. This was the object which M. Lagrange proposed to himself. He analyses all the known methods, and shows their uncertainty and insufficiency. He reduces the problem to the determination of a quantity smaller than the smallest difference between the roots. This is something. We cannot too much admire the analytical skill displayed throughout the whole work. But notwithstanding all the resources of the genius of M. Lagrange, we cannot conceal that the labour of his method is exceedingly great, and calculators will doubtless continue to prefer methods less direct indeed, but more expeditious. The author resumed this subject no less than four times. It is to be feared that a commodious and general solution will never be discovered, or at least it must be sought for by other means. The author seems to have acknowledged this himself, as he recommends the method of M. Budan as the most convenient and elegant for resolving equations whose roots are all real.

The desire of multiplying useful applications induced him to undertage a new edition of the Mecanique Analytique. His project was to develope the most useful parts of it. He laboured at it with all the ardour and intellectual power which he could have applied at any period of his life. But this application occasioned a degree of fatigue which threw him into a fainting fit. He was found in that state by Madame Lagrange. His head in falling had struck against the corner of a table, and this shock had not restored him to his senses. This was a warning to take more care of himself. He thought so at first; but he was too anxious to finish his work, the printing of which is at present at the twenty-sixth sheet of the second volume. The first volume had appeared some time before his death. It had been followed by a new edition of his Fonctions Analytiques. So much labour exhausted him. Towards the end of March a fever came on, he lost his appetite, his sleep was uneasy, and his waking was accompanied by alarming swoonings. He perceived his danger; but, preserving his undisturbable serenity, he studied what passed within him, and, as if he were assisting at a great and uncommon experiment, he bestowed all his attention on it. His remarks have not been lost. Friendship conducted to his house on the 8th of April, in the morning, MM. Lacepede, Monge, and Chaptal, who took care to write down the principal points of a conversation, which was his last. (We have scrupulously followed these notes, and the passages under inverted commas are faithfully copied from the manuscript of M. Chaptal.)

"He received them with tenderness and cordiality. I was very ill, my friends," said he, "the day before yesterday; I perceived myself dying, my body became weaker, my moral and physical powers were gradually declining; I observed with pleasure the gradual diminution of my strength, and I arrived at the point without pain, without regret, and by a very gentle declivity. Death is not to be feared, and when it comes without violence it is a last function, which is neither painful nor disageeable." Then he explained to them his ideas respecting life, the seat of which he considered as spread over the whole body, in every organ and all parts of the machine, which in his case became equally feebler in every part by the same degrees. "A little longer, and there would have been no functions, death would have overspread the whole body, for death is merely the absolute repose of the body; I wished to die," added he with greater force; "I found a pleasure in it; but my wife did not wish it. I should have preferred at that time a wife less kind, less eager to restore my strength, and who would have allowed me gently to have finished my career.

I have performed my task, I have acquired some celebrity in the mathematics, I have hated nobody, I have done no ill ; it is now proper to finish."

As he was very animated, especially at these last words, his friends, notwithstanding the interest, with which they listened to him, proposed to retire. He retained them, began to relate to them the history of his life, of his labours, of his success, of his residence at Berlin, where he had often told us what he had seen near a King ; of his arrival at Paris, the tranquillity he had enjoyed at first, the anxiety occasioned to him by the Revolution, and how he had been finally rewarded by a powerful monarch, capable of appreciating his worth, who had loaded him with honours and dignities, and who had even lately sent him the Grand Ribbon of the Imperial Order of Re-union. Let us add likewise, who after having given him during his life the most unequivocal proofs of the highest esteem, has since done more for his widow and his brother than ever Frederic had done for him while he was Director of his Academy.

He had neither been ambitious of riches nor honour ; but he had received both with respectful gratitude, and rejoiced at the acquisition for the advantage of the sciences. He meant to affix these titles to the fronticepiece of his work, " in order to show the universe to what a degree the Emperor loved and honoured philosophers."

From these last words we see that he had not lost all hope of cure ; he believed only that his convalescence would be long. He offered, when he recovered his strength, to go and dine at M. Lacepede's country house with MM. Monge and Chaptal, and proposed to give them details respecting his life which could be found nowhere else. These details are irretrievably lost. We do not even know to what he alluded, nor what he could have added to the second volume of the Mecanique Analytique, which was then in the press. We have just learned that the Countess Lagrange has put into the hands of M. Prony the complete manuscript of the second volume, in which will be found important additions, and sections entirely written anew. By the care of an editor so skilful, and so devoted to the memory of the author, the philosophical world is sure of obtaining with the greatest accuracy and despatch what is wanting to complete the work, and perhaps even memoirs entirely new.

" During this conversation, which lasted more than two hours, his memory often failed him ; he made vain efforts to recover names and dates, but his discourse was always connected, full of strong thoughts and bold expressions." This exercise of his

faculties wasted the whole remains of his strength. Scarcely had his friends left him, when he fell into a fainting fit, and he died two days after, on the 10th of April, 1813, at three quarters past nine o'clock in the morning.

M. Lagrange was of a delicate but good complexion. His tranquillity, his moderation, an austere and frugal regimen, from which he rarely deviated, prolonged his life to the age of seventy-seven years, two months, and ten days. He was twice married: first at Berlin, in order to be on a footing with the rest of the academicians, none of whom were bachelors. He brought from Turin one of his relations. He married her, and lost her after a long illness during which he had bestowed on her the most tender and unremitted care. When he afterwards married, in France, Mademoiselle Lemonnier, daughter of the celebrated astronomer of that name, he said to us, "I had no children by my first marriage; I do not know if I shall have them by my second; but I scarcely desire them." What he principally wished was an amiable companion, whose society might afford him some amusement during the intervals of his studies, and in this respect he was very successful. Madame Lagrange, daughter, grandaughter, and niece, of members of the Academy of Sciences, was deserving of the name which he gave her. This advantage in her eyes making up for the difference of their ages, she soon felt for him the tenderest regard. He was so grateful that he could scarcely bear to be separated from her, and it was on her account alone that he felt any regret at relinquishing this life; and he was often heard to say, that of all his good fortune, that which he prized the most was having obtained a companion so tender and attached to him. During the ten days that his illness lasted she never quitted him for a moment, and was constantly employed in recruiting his strength and prolonging his existence.

He loved retirement; but did not insist upon his young wife's following his example. On her account he went out more frequently, and indeed his high situations obliged him to show himself in the world. It was often apparent that he continued the meditations in public which he had begun in his cabinet. It has been said that he was not insensible to the charms of music. In fact, in a numerous company he was not displeased at a concert. On one of these occasion I asked him what he thought of the music: "I love it," says he, "because it leaves me to myself. I listen to it during the first three measures, but I hear no more of it; I give myself up to reflection, nothing interrupts me, and in this way I have solved many a difficult problem." Hence the finest music must have been that during which he was inspired with the finest of his thoughts.

Though he had a venerable figure, indicating his excellent characteristics, he would never allow his portrait to be drawn. More than once, by a very excusable piece of address, persons have been introduced during the meeting of the Institute to take a *sketch of him without his knowledge.* An artist sent by the Academy of Turin drew in this manner the outline from which was constructed the bust that was exhibited for some months in the hall of the Institute, and is at present in the library. A cast was taken of him after his death; and some time before, while he slept, a picture of him was taken, which is said to resemble him very much.

Gentle and even timid in conversation, he took a pleasure in asking questions, either to draw out others, or to add their reflections to his own vast knowledge. When he spoke, it was always in a tone of doubt, and his first words usually were, *I do not know.* He respected the opinions of others, and was very far from laying down his own as a rule. Yet it was not easy to make him change them. Sometimes he even defended them with a degree of heat which continued to increase till he was sensible of some alteration in himself; then he immediately resumed his usual tranquillity. One day, after a discussion of this kind, M. Lagrange having left the room, Borda remaining alone with me, allowed these words to escape him: "I am sorry to say it of a man like M. Lagrange, but I do not know a more obstinate person." If Borda had gone away first, Lagrange might have said to me as much of our associate, who was a man of excellent sense and considerable wit; but who, like Lagrange, did not easily abandon those opinions which he had adopted after a mature examination.

A gentle and good-natured irony was often remarkable in the tone of his voice; but I never saw any person hurt at it; because it was necessary to have well understood every thing that went before to perceive the true intention of it.

Among all the master-pieces which we owe to his genius, his Mecanique is certainly the most remarkable and the most important. The Fonctions Analytiques hold only the second place, notwithstanding the fruitfulness of the principal idea, and the beauty of the developments. A notation less commodious, and calculations more embarrassing, though more luminous, will prevent mathematicians from employing, except in certain difficult and doubtful cases, his symbols and names. It is sufficient that he has proved the legitimacy of the more expeditious processes of the differential and integral calculus. He has himself followed the ordinary notation in the second edition of his Mecanique.

This great work is entirely founded on the calculus of variations,

of which he was the inventor. The whole flows from a single formula, and from a principle known before his time ; but the whole utility of which was far from suspected. This sublime composition includes all his other preceding labours which could be connected with it. It is distinguished likewise by the philosophical spirit which reigns from one end of it to the other. It is likewise the best history of that part of the science, a history which could only have been written by a man perfectly master of his subject, and superior to all his predecessors, whose works he analyses. It forms a most interesting piece of reading even to him who is not capable of appreciating all the details. Such a reader will at least find the intimate connexion of all the principles on which the greatest mathematicians have founded their researches into mechanics. He will there see the geometrical law of the celestial motions deduced from simple mechanical and analytical considerations. From those problems, which serve to calculate the true system of the world, the author passes to questions more difficult, more complicated, and which belong to another order of things. These researches are only objects of pure curiosity, as the author announces, but they show the extent of his resources. Finally, we see there his new theory of the variations of arbitrary constant quantities of the motion of the planets, which had appeared with so much eclat in the Memoirs of the Institute, where it had shown that the author, at the age of seventy-five, had not sunk from the rank which he had filled for so long a time in the opinions of all mathematicians.

In every part of his writings, when he makes use of an important theorem, he names the original discoverer of it.

When he opposes the ideas of his predecessors and contemporaries, it is with all the attention due to genius ; when he points out the errors of those who have attacked him, it is with the apathy of a true mathematician, and the calmness of a demonstration. None of his celebrated rivals had ideas more just, more fine, more general, and more profound. Finally, thanks to his happy labours, the science of mathematics is now like a vast and fine palace, the foundations of which he renewed, and in which we cannot take a single step without perceiving admirable monuments of his genius.

ART. XXV.—*Some Account of the Climate &c. of the North of France, collected partly from Observation, partly from a free Communication with the Inhabitants of various ranks.* Written, during a Residence in that Country, for the use of a Friend in Britain. By H. H. BLACKADDER, Esq. Surgeon. [*Brewster's Journal.*]

THE northern part of France distinguished more exclusively by the name of French Flanders, comprehends about 60 square leagues, or 540 square miles,—extending N. E. to W., from Dunkirk to Calais, about nine leagues, and N. to S. E., from Gravelines to Cassel, about eight leagues. This tract of country forms one extensive level plain, which was, beyond all doubt, at some remote period occupied by the sea. The insulated hill on which Cassel stands, and which rises from five to six hundred feet above the level of the plain, was at one time bathed by the sea, from which it is now distant about six leagues,—and there are monuments which seem to indicate that vessels could in former times reach even to St Omer, which is eight leagues from the present shore of the English Channel.

The period, at which the land began thus to encroach upon the sea, is lost in the lapse of ages,—all that we can discover from history is, that this encroachment had made considerable progress anterior to the age of Julius Cæsar. No fossil shells are found in the soil, but trees, similar to those found in Holland, have been dug out sixteen feet below the surface. The cause of this formation of land, and consequent repulsion of the sea, seems to be the successive accumulation of alluvial matter brought down by the rivers,—including, perhaps, the Rhine, the Meuse, and the Scheldt, along with the action of the North Sea on the coast of Holland. Gravelines, built at the mouth of a river, was, at the beginning of the last century, bathed by the sea, and is now distant from it about three miles.

The surface of the country is but very little elevated above the level of the sea at low water; and when the sea is full, the land is so much below it, that were it not for the artificial embankments along the shore, and if the sluices were not regularly shut at Calais, Gravelines, Dunkirk and Fort Nieulay, the whole country would be inundated. Hence it is that the rivers and canals do not flow during high water, a phenomenon which always attracts the attention of stangers, more especially when unacquainted with the cause.

Strictly speaking, there is but one river that passes through this

country, which is named the Aa, and which has its origin near Reuti in Artois, about six leagues to the S. W. of St Omer. Passing through the last mentioned town, its direction is towards Gravelines, and about a league beyond that place it reaches the sea,—its whole course not being less than sixty or seventy miles. At different parts, as in the vicinity of St Omer, the bed of the river has been raised many feet above the surrounding fields, so that it is impossible to divine what its original distribution may have been. At Waten it gives off a branch called the Colme, which runs N., passes Bergue, and enters the sea at Dunkirk. A second branch goes towards Bourbourg, which it surrounds and crosses, and, with the Colme, reaches the sea at Dunkirk. About two centuries ago these branches were enlarged, (being then only small streams,) so as to form two canals for the purpose of navigation,—and serving also more effectually to carry off the water from the innumerable small canals, (termed *watergangs* in the language of the country,) and from the ditches which communicate with them.

To a stranger, nothing is more novel and characterestic, than the manner in which this tract of country is divided and intersected by the small canals and ditches ; the meadows, corn-fields, plantations, and gardens of each proprietor, being divided by these watergangs into small and more or less regular squares, each of which is completely insulated, so as to admit of boats readily passing between them. The whole may be compared to the streets and alleys of a large city, where water and boats come in the place of pavements and carriages. The execution of these works has been of incalculable advantage to the country,—immense tracts of rich soil, otherwise worse than useless, has been rendered highly productive ; land carriage is almost unknown ; and last, but not least, the endemic diseases, which were formerly the scourge of the inhabitants, have now nearly disappeared.

The river Aa runs slowly on a slimy channel, and which is fit for forming peat.* Hence the water has a peculiar brackish

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* In many places peats are dug out of the watergangs, &c. several feet under the surface of the water. For this purpose, the person goes into a large half-decked boat, and is provided with a very long-handled spade, the rest for the foot being four or five feet from the cutting extremity. With this instrument he digs up the peat, which is easily raised to the surface of the water, but then requires a considerable exertion to place it on the deck. Afterwards the peats are transferred to the land, to be thoroughly dried, and where, for a considerable time, they emit a most offensive odour. When dry they are very different from the peat to be met with in Scotland. They are very hard, black, and ponderous,—not easily kindled, and in burning, give out a most disagreeable and almost unsupportable odour. This odour is well known to strangers who may have walked round the old ramparts of Calais, though they may have remained ignorant of its true origin. The cause is doubtless

taste, which is very disagreeable. Over the whole of this country, indeed, the water is sufficiently bad ; but that of the Colme and the canals is even worse than that of the Aa. In general it has a muddy brown colour, and deposits a greenish yellow slime in great abundance. Many of the inhabitants are nevertheless under the necessity of using water from the watergangs and ditches, however unwholesome. It is very dangerous to make use of these waters as drink, but their offensive taste commonly prevents their use in that form, the drink of the inhabitants being always a kind of tea or beer. Much use is made of water from draw-wells, though the quality of this is also very bad, being strongly impregnated with saline and other substances. It is more discoloured, and of greater specific gravity, than the water of the rivers and canals, and is unfit for culinary purposes, animal and vegetable substances when concocted in it, acquiring a horny consistence. The principal use to which the water of draw-wells is applied is the washing of the houses, a practice to which the inhabitants are, to appearance, excessively addicted. But, perhaps, if their houses were not thus kept proportionably damp, the inmates would be less able to resist the excessive dampness of the external air, to which they are liable to be exposed in all variety of circumstances.

It is much to be regretted that cisterns, from which so much advantage has long been derived in Holland, are here almost unknown.

The soil seems to be of the same description over the whole of this tract of country. It is very moist, and is composed of a mixture of clay, silicious earth, peat-moss, and a considerable proportion of animal and vegetable substances in a state of decomposition. So much do the latter substances abound, that, when it is examined in the hand, it has that over-rich, or rather greasy appearance of the soil, that is to be found in the vicinity of old dunghills. Hence it is that ashes are in such great repute as a manure, and are sold at a high price. The cultivated fields are highly productive, and the pastures and meadows, especially those on the banks of the canals, around the villages and large farm-houses, are rich even to luxuriance. There are also considerable plantations, the vegetation of which is strikingly rapid, many trees acquiring large dimensions in the course of a few years. The progress made after engrafting is also remarkable ; in some in-

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to be traced to the great quantity of decayed animal matter that enters into the composition of this species of peat. As the canals afford a ready and cheap mode of conveyance, and as both wood and pit-coal are high priced, these peats are much used by the lower ranks, both in the towns and villages, as well as in the country.

stances, indeed, that I had an opportunity of observing, it exceeded any thing I could have imagined in so northerly a climate. It may be observed, that the trees incline to the east, and that on their sides next to the sea there are comparatively few branches. The agricultural productions are, in general, of the best quality; wheat and rye in great abundance, a great quantity of these being sent to the interior. The diseases which affect the corn, are said to be le charbon, la nielle, la rouille, le blé moucheté; the ergot of the rye is of very rare occurrence. It is not uncommon, excepting in the summer months, to see the meadows, pastures, and, in some places, even the cornfields, covered with water. Hence it happens, that when the autumn is particularly rainy, the sown fields are ruined, and it becomes necessary to re-sow them in the month of March. Potatoes are in general cultivation, though not so much used as in Britain, excepting on occasions of scarcity. There are some varieties to be met with in the markets. The first is characterized by the paleness of the skin and whiteness of the flesh, resembling in these respects the variety named *white blooms* in some parts of Scotland, but in other respects it is much superior. It is, upon the whole, rather tasteless, and is most apt to be injured by frost. The skin of the second has a fine claret colour, and the flesh, sufficiently dry, is more compact than that of most other varieties. It seldom exceeds the size of an egg, has an agreeable flavour, and is much used in soup along with other vegetables. The third is an excellent and beautiful variety of the kidney species. The skin is of a bright claret colour, which extends into the flesh. It grows from two to five or six inches in length, and of uniform thickness, which is seldom so much as an inch. This potatoe is very dry, has a very agreeable flavour, and though, with other varieties of the kidney species, it is not very productive, it is surprising that it is not met with in our gardens. It is usually the first that appears in the market, and must be considered as an early variety. The fourth is that named the black potatoe, though its flesh be the whitest of all. This, as elsewhere, is cultivated for spring use, being universally recognised as withstanding the frost, and as being later in vegetating than any other variety. I never observed any appearance of disease in the potatoes raised in this country.

The horticultural productions are very abundant, and, upon the whole, rather luxuriant, approaching to that which is termed *rank*—the quantity and size being much more characteristic than the richness of the flavour. Still, however, the cherries, currants, apples, pears, peaches, plums, and other fruits, form a great part of the wealth of the inhabitants. Gooseberries are not much

cultivated, and are chiefly used in an unripe state. Much use is made of roasted apples and pears, more especially in the time of Lent. The coarsest apples and pears, which are otherwise only fit for making cider and perry, become pulpy, and of a fine sub-acid taste, by simple roasting in the oven. In this state they are preserved close packed in jars for almost any length of time, and without the addition of sugar. Spinage and sorrel are also preserved in jars, so as to furnish a constant supply at a trifling expense during winter and spring,—a little salt and half boiling of the leaves being all that is necessary. For the purpose of promoting and protecting early vegetation, I find much advantage is derived from the use of easily moveable hurdles, made by placing a thin layer of straw or reeds longitudinally between thin wicker-work, and which can be constructed at almost no expense. These are placed in various directions around the beds, and shifted according to circumstances. There is one method of having early salad, which is very simple. They sow the lettuce seed very thick, and when the young plants are about an inch in height, they are thinned out. These young plants make a most delicate salad, and the taste can be rendered more piquant by the addition of a few plants of the *Lactuca virosa*, or by a little endive, or blanched dandelion.

On the sea-shore is found the *Critim. marit. maj.*, which, when confected in vinegar, is an article of commerce.

The prevailing quality of the air is humidity, the face of the sky being commonly concealed by a sombre greyness of the atmosphere, at no great height above the earth. The barometer is variable, seldom remaining of the same height for two days in succession. The extremes are said to be 26, 3′—29. It has been remarked, that the changes are more frequent and more rapid than in the interior, and that the variations are most remarkable about the equinoxes. The mean temperature is about 5° above that of Paris. The cold season is of long duration, but is commonly not severe. Judging from the animal sensations, however, this climate is colder than that of Paris, which must arise from the great dampness of the air, caused by the vicinity of the sea, the number of canals and ditches, the lowness and consequent moistness of the soil, and the great luxuriance of vegetation.

When the south or west winds continue for some time in any season, abundant rain is the consequence. The animal system then feels relaxed and oppressed; the circulation is slow, and the secretions interrupted. Persons feel themselves heavy, and incapacitated for either mental or corporeal exertion, and many, but particularly strangers, acquire a dingy yellow colour of the skin,

obviously connected with some irritation of the biliary organs. The state of the atmosphere is quickly changed by a north or northeast wind coming from the sea. These winds are dry and invigorating, but not unfrequently accompanied by a dense fog. The winds are very variable, often changing in the course of the same day. The most frequent are the southwest and the north, the most rare the east. The southwest and north winds are always the strongest, and the south wind is very moist. The southwest wind is common in autumn, the north wind in spring.

The number of serene days is about forty, and occur only during a very hot or very cold state of the air. Most commonly the sky is covered with a universal greyness, through which the sun only occasionally makes his appearance.

They count, on an average, about twelve thunder storms in the course of the year ; and not unusually a tremendous one all at once ushers in the winter. In a storm of this kind which I witnessed, a wind-mill, at a short distance, was struck by the electric fluid, and immediately after the flash, there suddenly fell a shower of angular pieces of ice, by which some individuals had their faces slightly lacerated.

Heavy fogs are very frequent, often appearing at mid-day, and extending from the sea, envelope all Flanders, but seldom advance interiorly beyond the province of Artois. These fogs occur occasionally at all seasons, but are most common in spring and autumn.

The quantity of rain is only somewhat greater than at Paris. There, the mean is 24 inches 2 lines, and there are 119 days with rain ; here, the mean is 24,4, and the number of days with rain 159. May, July, August, and November are the months in which most rain is observed to fall.

In spring, the north wind is almost constant, especially from the beginning of March to the middle of April. This wind is strong, dry, cold, and sharp. It freezes in the night, and the barometer always keeps high, but the sky is uniformly obscured. Hoarfrosts are apt to occur until May ; and it is not till the middle of that month that the air begins to feel sensibly and steadily warmer.

In summer, the weather is very variable, there being continual changes from heat to cold, and *vice versa*, not only from day to day, but in the course of the same day. A fog coming in a direction from the sea, extends all over the country, and then the thermometer sinks. In July and August the heat is considerable, sometimes so high as 92°. On such occasions there is always a perfect calm, but this state of the air never lasts long ; clouds soon begin to make their appearance, and the thermometer falls to about

65°. The usual summer heat is from 65° to 80°. At this season there is always a heavy dew during the night, accompanied by a very cold and dense fog, the thermometer falling from 8° to 14° lower than during the day. Hence the origin of various diseases, to which persons are liable when exposed to the open air during the night. No where is the sudden sensation of cold at sunset more remarkable.

In autumn, the weather is more steady ; after the equinox, the thermometer sinks to 50°, and vacillates but little for a considerable length of time. Frosts do not usually commence till December ; but during the night and part of the morning, there is always a dense cold fog, which moistens all the surface of the ground, and of solid bodies resting on it.

In winter, the temperature rarely falls so low as 20°, but it has once or twice been observed nearly at 0°. In the winter of 1819, I observed it so low as 10° a considerable time after sunrise. There had previously been a great fall of snow without wind, and there was now a gentle but steady northerly breeze. The whole sky was free of clouds, had a deep blue colour, and a peculiar glistening appearance, produced by a minute icy spiculæ with which it was heavily charged, and which, impinging on the face, in walking against the wind, were sufficient to blister it. These spiculæ had no effect on the appearance of the sun, if it was not to render the light more intensely white and glistening.

A humid atmosphere, habitual heavy fogs, a moderate temperature, a low and moist soil, the free use of watery drinks, such as beer, the daily use of a great quantity of milk and butter, an abundant nourishment of wheat-bread, butcher's meat, tea, and fresh water fish, and garden vegetables of all kinds, the ease of all ranks of society, and labour to occupy all hands, all these things united must greatly influence the physical and intellectual constitution of the inhabitants. In general, men, animals, and vegetables, are large. A moist climate, and a fertile soil, are favourable to the developement of all the parts of animate beings. Hence we remark large bones, large muscles, with a profusion of cellular substance. When King Henry the VIII. described a certain princess as " a great Flanders mare," the comparison was rude, but sufficiently intelligible. In general, the complexion of the inhabitants is fair, the eyes blue, and the hair chesnut. They support labour well, and often arrive to an advanced age. In character they are a mixture of the Frenchman and Hollander, and which, when closely anlayzed, is found to be rather an odd compound.

Beer of various qualities is much used, and spirituous liquors (particularly gin, which sells at the rate of about sixpence the

bottle) are consumed in great quantity. Drunkenness, however, even in the towns, is rare, very rare, compared with what may be observed in Britain, and habitual drinkers still more so. But it is an almost universal practice to take a glass of raw spirits on first rising in the morning, a practice common, and said to have been found beneficial, in most damp countries on the continent of Europe. The use of spirits mixed with water, in the form of grog or punch, is all but unknown, such a mixture being considered highly pernicious to the stomach, and it would be well, perhaps, if all their opinions were equally well founded. Tea, with or without milk, but always without sugar, a weak beer, and "bouillie," are the common drinks. As formerly mentioned, the water cannot be used in its crude state with impunity. The bouillie is made from a decoction of bran, to which old leaven has been added; fermentation ensues, and the product is a refreshing subacid liquor, which removes thirst very well.

Milk, butter, cheese, and bread, constitute the principal nourishment of the country people. Even the common people never eat bread without butter (excepting in Lent), and he who is reduced to do so, is considered at the depth of misery. Hence the proverbial expression, "*Manger le pain sec.*"

There is a mess called sour-milk much used by the country people, and which is made as follows: A considerable quantity of milk is put into a deep wooden vessel, and a certain quantity of salt is added to it. It is then left until the whey separates from the curd, when the former is poured off and given to the pigs, and the latter is stirred round, and more milk added to it. This operation is repeated until the desired quantity of curd is obtained, and which is found to have acquired a very acid taste. In this state it is kept for winter use, and is used in mixing a quantity of it with water and flour, which is boiled, and then bread is added to it. This mess is used for breakfast and supper, which always concludes with bread and butter.

There is another mess of curd often used in the summer months, and much relished for supper. For making it, two vessels are provided, the one of which goes within the other, the innermost being perforated with numerous holes for the escape of the whey. The milk is coagulated by means of runnet made by infusing a small piece of the dried stomach of a young hare in white wine. To the curd, well freed of the whey, is added salt and pepper, but many consider eschalots an indispensable ingredient.

In the country the houses are built of mud or brick, and roofed with thatch. In the towns, they are built of brick, and roofed with flat tiles, which at least have a better appearance than those

used in Scotland, but they require the roof to be covered with deal, the same as in the case of slates. Nothing is more agreeable than the internal appearance of these houses ; every where there is exhibited, as in Holland, an exact attention to cleanliness ; but the means employed for attaining this is far from being agreeable to strangers. Several large tubs of cold water are emptied in each apartment, and when every part has been well scrubbed with a birchen broom, the remaining water is swept out by the door, and in this state it is left to dry, which, in this climate, is a very slow process. This deluging operation is of perpetual recurrence, and hence those who are not completely accustomed to this moist manner of life, are continually exposed to attacks of rheumatism, toothach, &c.

Occasional inundations from heavy falls of rain are liable to take place, and when the water is nearly evaporated, there are elicited vapours of a very noxious quality. Notwithstanding the numerous canals, &c. there still exist some marshes, and in hot weather their effluvia are very pernicious. Some of these marshes are formed by digging peat, and clay to make bricks ; others have existed from time immemorial. At each farm-house there is a pond of no small extent, to receive the fluid part of the dung-hills, &c. These are of a greenish or reddish brown colour, and in autumn give out a most abominable odour. Strange as it may appear, however, almost every farmer sends his cattle to drink out of these ponds, and even the animals themselves seem to prefer this fluid (for water it cannot be called) to the water of the canals and ditches. It must be admitted, that there are no direct proofs of the use of this fluid as drink, being prejudicial, and one thing is certain, that these ponds must be almost, if not altogether free, of insects and animalculæ, which literally swarm every where else, whether in air, earth, or water.

The immense quantity of small animals of one kind and another, that are to be seen in and about the canals, ditches, and fields, enables us to account for no small portion of the decayed animal matter that is so abundantly mixed with the soil. Frogs are very abundant, but though they are regularly brought to the market in Brussels, I never saw or could hear of their being used as food in this country. At some places leeches are bred in the marshes, and during the war, a great deal of money was made by the sale of them. A common mode of collecting these animals deserves to be noticed, as it explains a fact which has often caused suspicion and blame to attach to upright leech-dealers in the interior, and in Scotland not less than elsewhere. A number of persons, commonly young women, are employed to wade in the water

with their feet and legs uncovered, and when they come out after a short stay, these parts of their body are found covered with leeches, which are then removed and secured in vessels provided for their reception. This operation they repeat as often as may be requisite for obtaining the desired quantity, and, far from suffering any injury from the loss of blood, these Flemish water nymphs informed me that their health was always improved by it at the time; that their complexions became clearer, and that afterwards they got more into a state of *em bon point*. It must be obvious, that leeches, caught in this manner, must acquire a small quantity of blood, but when afterwards they are sold for use, and when some of this blood, still remaining, happens to be ejected on the application of the animal, it is forthwith taken for granted, that the dealer has acted dishonestly in selling used leeches for fresh ones.

Hares, partridges, and snipes, are in great plenty. Quails were once common, but are now become exceedingly rare. In the river are trout, which I have seen from two to three pounds weight, but their flesh is not delicate, approaching somewhat to that of the pike, which is, no doubt, owing to the gross slimy quality of the water. Artificial flies for the purpose of fishing are here unknown; —an excellent proof this that necessity is the mother of invention, for here artificial flies are unnecessary. At almost every step along the river is to be found the fly most proper for ensnaring the trout, and as it is neither small nor delicate, and as the river moves slowly, it is not soon torn from the hook. The fly is often seen to rest on and float down the stream when seized by the fish, and the fisher has only to imitate this dull operation. In the canals and ditches are pike, and an abundance of perch and other small fish. I have been informed that, about twenty years ago, a pike was killed of the enormous weight of thirty-two pounds; no one seemed to doubt the fact, but I have met with no one who had himself seen such a fish.

The canals and ditches, from time to time, become more or less choked, from the continual deposition of alluvial matter, and, when cleared out by the workmen, the matter ejected yields a most unsupportable odour. As the riches of the country depend on the keeping of these canals in repair, they are cleaned out at least once every two years, besides other occasional repairs; and there is a class of workmen who nearly confine themselves to this particular occupation, which is none of the most healthy. It is in autumn, winter, and spring, that these operations are carried forward, and the matter ejected is spread over the fields as manure. Nothing of the kind can be more offensive to the smell, and few more pernicious to the health, than the vapour which continues for a

long time to exhale from it. Still, if we suppose a foreign army to take possession of this country, it would be of the first importance to protect the inhabitants, and give every possible encouragement to the regular execution of these works, otherwise they would soon and dearly pay for their imprudent conduct.

On the sea-shore the inhabitants are, in general, more healthy than in the interior; but when the sea happens to break over the dikes, and the water afterwards comes to be evaporated, exhalations of an extremely pernicious quality are elicited, and which cause fevers of the very worst kind.

Intermittent fevers are not so frequent here as on the coast of France, in the vicinity of Rochfort. In this country the ditches are less frequently dry, the temperature being considerably lower, and the air in common much damper, the evaporation is much less active. The adult inhabitants are but little subject to the attacks of the endemic fevers, but children and strangers rarely escape. They are commonly attacked in autumn; at first it appears under the form of a tertian, afterwards it changes to a quartan, and about January, some change to tripple quartans, and instances to quotidians. From the middle of April to the middle of May, these fevers, when not interfered with by medical treatment, cease of themselves. The patients, however, are liable to a recurrence of the disorder in the course of the ensuing summer; and nothing is more common than to see children who have had the fever hanging about them for two or three years in succession, during which time they preserve their appetite, and even eat more than when in perfect health. It is not customary to treat this disease medically in the case of children, and accidents are rare. When a child has once had and got quit of the fever, he is thereby *acclimaté*, or rendered less liable to suffer from the climate in the after period of his life, if he be not much exposed to the exciting cause, such as digging the ditches. Of all others, the men who mow the hay suffer most; the ditchers are all natives, but the hay-makers come from a different and higher part of the country, being allured by the love of gain. As they always work at what is termed piece-work, their exertions are great, and as the hay falls, the dank surface of the soil, consisting of the half decomposed ejectment from the diches, is exposed to the direct rays of the sun: Hence it happens that the noxious vapour arises most abundantly at a time when the men are exhausted with labour, and consequently when the system is least able to resist its influence. These labourers contract complicated tertians, and, in the language of the country, are said to have "*swallowed the frog in Flanders.*" The patient experiences a feeling of great debili-

ty ; the secretions seem almost suspended ; the respiration is slow and oppressed ; and the surface of the body assumes a leaden colour. The first care of these unfortunate persons is to quit the marshy country, and when they get home, and are properly attended to, they often rccover, though some occasionally sink under various supervening complications.

Upon the whole, this is a very rich and populous country, and in several respects, very highly interesting ; but it is neither very agreeable nor very healthy, especially to strangers, as a place of residence. Catarrh and rheumatism are frequent in autumn and spring. In summer diarrhœa, verging to dysentery, is not unfrequent. In the towns, chronic ophthalmia is pretty common, and there appears to be an unusual proportion of halt and lame, and that apparently from their youth.

ART. XXVI. *Essay on Calico-printing.* [*Parkes' Chemical Essays.*]

As the whole of this ingenious business, as it is now conducted, depends upon the proper application of a few compounds called mordants, it will be necessary, in the first place, to explain their nature and uses. In doing this, one or two preliminary remarks will assist us.

The colouring substances chiefly employed in this art, are divided into two classes, viz. *substantive* and *adjective*. A SUBSTANTIVE colour is one which is capable of itself of producing a permanent dye ; such is the juice of the buccinum,* used by the ancients for producing the imperial purple ; such are also the woad and indigo† employed by the moderns for producing a permanent blue ; and we may add the metallic solutions, particularly those of iron, cobalt, gold, platinum, and silver, which give various colours, according to the processes by which they are prepared.

By ADJECTIVE colours are meant all those which are incapable of giving permanent dyes without the aid of certain intermedia,

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* It has been proposed to employ this valuable permanent colour for penceling on fine muslins. I believe it might readily be procured in sufficient quantities, and would prove an important addition to the resources of the British calico-printer.

† Dr Bancroft tells us, that the first mention of indigo, as known in England, is in the Act of the 23d of Queen Elizabeth, chap. 9, where it is called *Ancle*, or *Blue Inde*. Bancroft *On permanent Colours*, page 138.

which form as it were a bond of union between them and the substances intended to be dyed.

These intermedia are what are known by the term MORDANTS, and they are used for this purpose in very considerable quantities by the calico-printer of the present day.

"The ancient Gauls and Britons were not ignorant of the method of imparting various dyes by means of substantive colours; we have even direct evidence that they excelled in some branches of this art, and possessed valuable secrets in it that were unknown to other nations. Several of the herbs which the Gauls and Britons used in dyeing are occasionally mentioned by Pliny, in different places; but the herb which they chiefly used for this purpose was the *glastum* or *woad*, the isatis of Linnæus; and they seem to have been led to the discovery of its valuable properties in dyeing cloth, from the former use of it in painting and staining their bodies."*

Several expedients were also employed by the ancients to produce fast or, more properly, permanent colours, by means of mordants, as appears from the testimony of Aristotle and Pliny. The chief articles in use at present in Great Britain, are the acetate of iron, the acetate of alumina, and the various solutions of tin, all of which should be very carefully and correctly prepared.

When piece-goods are designed to be dyed of one uniform adjective colour, they are first immersed in a solution of one of these mordants and are then hung up to dry, and to absorb the oxygen of the atmosphere. When sufficiently exposed to the air, they are washed or dunged,† to remove the superfluous mordant; that is to say, that part of it which is not chemically combined with the cloth; and the pieces are then submitted to a bath of that particular kind of colouring matter which is to be imparted to them.

It may here be remarked, that if a sufficient number of colouring substances should ever be discovered, that have no affinity for any thing but the chemical mordants, the business of calico-printing would be rendered much more easy and simple than it is at present.

Whenever it is meant that a colour should be partially inserted, the mordant is applied to those particular parts only; so that, when the piece is immersed in the colouring bath, no other place

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* Henry's *History of Great Britain*, 8vo. vol. ii. page 128.—Pliny's *Natural History*, books 16, 18, 21, and 22.

† The dung of the cow is used in such large quantities by the calico-printer, that it has become an article of great expense. The proportion that is employed is usually about one bushel to one hundred gallons of water, though frequently a larger proportion would be more effectual. The brightness of the colours, and the purity of the whites, are always dependent upon the quantity of the dung employed.

will receive the permanent stain. For though the whole surface of the cloth will be coloured, yet having in itself no affinity with the vegetable with which the decoction is impregnated, the whole of the colouring matter will be easily removed by exposure to the air, and the ground of the piece restored to its original whiteness; while those parts to which the mordant was applied, will retain and fix the colours in a way which will be more fully explained hereafter.

Formerly all calico-printers were bleachers; but in the neighbourhood of London these are separate and distinct trades, and the printer either purchases bleached goods for printing on his own private account, or receives the cloth from his customers in a white state; and, when printed, he returns the identical pieces, and is paid so much per yard, according to the number of colours, for printing them.

Chloride or oxymuriate of lime is the agent generally employed in bleaching; but it appears to me that some other article might be introduced with advantage. For, as the goods are washed in diluted sulphuric acid when they are taken from the oxymuriate of lime, a sulphate of lime is always formed, which becomes fixed in the fabric, and, acting as a mordant when the pieces come into the madder copper, occasions an indelible stain, which in very fine goods often impairs their beauty. If oxymuriate of soda were employed, the sulphuric acid would form a soluble salt with the soda, easily removeable by washing. The expense of this article will, however, be always an objection to its use, except for rich and valuable prints.

I am aware that some printers, who are more anxious about the quantity than the quality of the work they perform, would deride this caution, and say they can obviate the difficulty by washing the goods *before* they are submitted to the sulphuric acid. But I am so confident of the extreme difficulty there is, even in this way of removing the oxymuriate of lime, that I know the inconveniences above mentioned will very often occur, almost inevitably.

In my opinion every printer should bleach his own goods, for it is impossible always to rely with confidence on the care of those who bleach for hire; and every printer knows that good bleaching is absolutely a necessary preliminary in the production of good printing. Indeed, this is now pretty generally acknowledged in the north of England; for most of the opulent houses in Lancashire and in Scotland, who produce fine work, are bleachers as well as printers.

No people have taken more pains to excel in bleaching than the

Irish, and their credit is established accordingly. The German linen is, I believe, generally better than theirs ; but the Irish has always the preference in foreign markets, owing to the superiority of the Irish in bleaching and finishing.

By whatever means the bleaching is performed, the printer commences his part of the business in the following manner.

The goods are first DRESSED by singing off the whole of the nap which is attached to them. This is effected by the following contrivance : Ten pieces are generally wired together, and wound upon a roller, from whence they are passed over a hot iron, nearly in the form of half a cylinder, and received upon another roller ; from thence they are returned to the iron, which is still kept red or nearly at a white heat. The use of repeating this process is to remove the nap more effectually than it would be done by passing it only once over.

The next operation is that of STEEPING, which consists merely in soaking the pieces for twenty-four hours in a vessel of weak alkaline lye, at a temperature of about 100°. These operations of singing and steeping are going on at one and the same time, which effectually prevents any accident that might otherwise arise from the effects of the hot iron.

The goods are then boiled or else bowked in a solution of potash ; (some workmen prefer to have this alkali in a pure caustic state ;) they are then well cleansed, by thorough washing in wash-wheels, or in stocks, to ensure their being entirely divested of the alkali. The intention of thus treating them with potash, is to remove any grease or impurity that may be attached to them, which would otherwise endanger the evenness and uniformity of the colours. This process is called ASHING.

By some observant calico-printers it has been imagined, that the rendering the lye caustic is apt to impair the texture of the cloth ; and I doubt not but that this has often been the case. Under the eye of the master, however, I am sure it might be employed with advantage and safety. With advantage, because *mild* alkali has but a slight affinity for the grease ; whereas *caustic* alkali forms a real soap with any oleaginous matter, and then the compound is readily soluble in water. And though the alkali be made perfectly caustic, if the solution be sufficiently diluted, it may be used with perfect safety.

It may be remarked, that in weaving calicoes the workman generally greases the reeds, in order to make them move easier. Whenever this grease is in the cloth, it becomes fixed by the operation of singing ; and if it be not taken out before bleaching, it will not come out afterwards by the usual process of ashing and

souring ; for, when the pieces are submitted to a blue vat to be dyed of a uniform self-colour, all those greasy places will be found to have taken the dye in a very imperfect manner. Tallow is also employed in dressing the warp, and this has a baneful effect on all goods which are designed for printing.

If, however, the calico-manufacturers themselves would make a point of preparing the oleaginous matter for the weavers, and would furnish them with nothing but pure VEGETABLE OILS, such as those of rape, linseed, &c., I think it very likely that these inconveniences would not occur ; because the stain from *vegetable* is not so indelible as that from *animal* oil.

This defect in the operation of the pale blue dipping is often attended with very serious consequences, so that I know of no subject which better deserves the patient investigation of the calico-printer. I have seen a large number of pieces of printed calicoes which had all taken a solution of iron so as to form an extremely good black ; also the acetate of alumina, which had formed with the madder a beautiful red ; likewise the citric acid, which produced a perfect resist—and yet, when these goods came into the blue vat, certain parts of the pattern intended for blue, still remained quite white, the indigo having had no action whatever on those parts ; a circumstance which is sufficient of itself to spoil the goods, and render the whole expense of printing the other colours totally lost.

To cleanse such goods previously to their being printed, various expedients have been adopted, but I apprehend nothing but a solution of caustic alkali can be depended upon. To prove the effect of any method which may be tried, it is a good way to run the pieces through water, and then to pass them from the water so gradually over a roller, as to give the superintendant an opportunity of examining every inch of the surface ; and if any part remains greasy, it will be seen at once, for that part will continue dry, while all the rest of the cloth is wet.

There is another way in which the goodness of bleaching might be proved. Let a few of the suspected pieces be run once or twice through a madder-copper, at the temperature of about 180°. This will inevitably mark any part that may be imperfectly bleached ; whereas, if the operation has been properly performed, they will come out so little stained, that an intelligent workman, who has been used to a madder copper, will at once be satisfied that they contain no impurity that can form a permanent mordant.

The next process in calico-printing is one with diluted sulphuric acid. A quantity of soft water having been poured into a leaden vessel, oil of vitriol is gradually added to it, in the proportion

of about twenty pounds of oil of vitriol to every hundred gallons of water, which by weight is in the proportion of about one to forty.

When this mixture has been well stirred, it is ready for use. Sometimes it is employed in this state, at others it is heated to 90° or upwards of Fahrenheit, according to the nature of the work to be done, and the goods are immersed in it. They are not suffered to lie in this acidulous liquor, but are wound by means of a winch over a wooden cylinder, that every part of the cloth may be successively immersed in the fluid, and every part exposed alternately to the action of the atmosphere.

This operation is generally continued for about twenty minutes, and is designed to remove any iron-moulds or other stains which the cloth may have acquired. It has also the effect of neutralizing any portion of potash that may have been left in contact with the cloth. The process is called SOURING.

After this operation it is necessary to wash the goods thoroughly, that no part of the acid may be left in them, to injure their texture; and this is best effected by means of the wash-wheel. The calicoes are then to be regularly and thoroughly dried, which finishes these preliminary operations, known in the trade by the term PREPARATION; so that those cloths which have passed through these manipulations are said to have undergone a preparation.

Besides the uses already mentioned, there is another advantage attending these processes, viz. that the cloth which has undergone this preparation will bleach sooner, the colours will be brighter, and the whites more delicate, than they would have been had they not gone through these previous operations.

The next process is that of CALENDERING. Here the goods are passed through a set of rollers, which gives them a gloss, and the appearance of their having been ironed. They are now fit for printing. But for copper-plate printing, or cylinder work, the process of calendering is omitted.

In printing fast colours, the artist usually proceeds in this way: He lays the piece of calico, which has been already smoothed by calendering, upon a strong thick table, which is previously covered with a woollen cloth. He then proceeds to apply one or more mordants, as the case may require, for fixing the intended colours. These mordants are applied by means of wooden blocks, with the patterns formed upon them. These blocks were formerly chosen of holly, or other hard wood, and the cutting them was a separate branch of the business. Of late years, however, a considerable improvement has been made in this department, by the introduction of brass or copper; that is, the pattern, instead of being actually cut in the wood, is now formed by means of slender pieces

of one of those metals being firmly fixed to the block, so as to produce the pattern intended. This alteration was occasioned by the perishable nature of wood, on account of which every printer has incurred great and unnecessary expense. The pattern when thus formed with copper, is not only more lasting, but it has also the advantage of giving greater sharpness and beauty to the impression.

When it was customary to use wooden blocks, the patterns were not enchased in the wood, but the wood was cut away in such a manner as to leave the pattern in relief. It will be obvious that this must always be the case in *block*-printing.

When the mordant is ready, it is mixed up either with flour-paste, or with a thick acqueous solution of gum arabic, gum senegal, or gum tragacanth.

Flour is an article of considerable consumption with the printers for making paste. Some houses buy twenty barrels of American flour at once. Should it be musty or sour from keeping, it is of little consequence for their use ; but they are careful to buy none but such as has been made with sound wheat, for if unsound it will be of no value for their purposes. Gum tragacanth is much dearer than the other gums ; but notwithstanding this, it must be had for some styles of work, as no other will answer for any of those colours or mordants which are prepared with nitrous acid. A solution of gum senegal would be coagulated in an instant by any of those preparations. Of late years an article called British gum has also been much in use for the same purpose ; so much so that the making of it has become a distinct and considerable trade. It is merely common STARCH calcined till it assumes a cinnamon brown colour, and then pulverized and passed through sieves of various degrees of fineness.

When the colour has been properly mixed with flour paste, or with a solution of either of the gums above mentioned, it is then spread upon a piece of superfine woollen cloth, strained tight upon a hoop.

This is placed within another hoop, covered either with sheep-skin or oil-cloth. These hoops are both so broad as to give to each of them the appearance of a tambarine. That which is covered with the woollen cloth is called a *sieve*, the other a *case*. The sieve within its case is now placed in a small tub of gum-water, and is ready for use.

When the apparatus is thus prepared, the mordant is applied by a brush to the surface of the sieve. This is called TEERING.

It should have been remarked, that when a colourless mordant, like the acetate of alumina, is employed, the workman generally

mixes a little of the decoction of Brazil wood, or of any other fugitive dye, with it. This is called SIGHTENING ; and is for the purpose of making the pattern more obvious to the workman, that he may see its progress, and the efficacy of the materials, as he proceeds in printing. The manipulation may be thus described.

Taking the block containing the pattern in one hand, the workman applies it gently to the surface of the sieve, so that a sufficient quantity of the thickened mordant may adhere to the figures. When the block is thus charged, he applies it to the calico, and gives it a blow with a small mallet, either slightly or otherwise, according to the nature of the pattern.

This alternate application of the block to the sieve and to the calico, is continued until the workman has gone over the whole piece. In this way several different mordants are sometimes applied to the same piece of goods. This is indeed always necessary, when the finished piece is intended to contain a variety of colours, the different colours requiring different mordants to fix them and render them permanent.

The calico is now removed to a room called the stove, where a certain degree of heat is given to it by means of flues, which go round the room on the inside, near the floor. In this room it is generally continued for at least twenty-four hours.* The intention of this is to evaporate the acids used in the preparation of the mordants, and which might otherwise injure the texture, and also to fix the base more surely within the fibres of the cloth.

Whenever acetate of alumina is printed as a mordant upon calico, it appears to me that the pieces ought not to go into the stove until the work has been first thoroughly dried in the open air, because it is well known that the earth of alumina is precipitated from a solution of this salt in a high temperature. I suspect this to be the cause of many of those disappointments which are perpetually occurring in the production of the various shades of yellow and madder reds.

In the operation of stoving, an attention to temperature is of the utmost importance. In general the room is kept at about 90° ; but an intelligent calico-printer varies this according to the nature of the work under operation.

It was formerly a practice to expose all those calicoes which were printed with the several mordants, to a hot stove for the purpose of evaporating the acids and fixing the base with more certainty in the fibres of the cloth ; but of late years a more conve-

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* This is when common red-liquor has alone been printed ; but if citric acid or strong muriate of tin has been employed, less time is sufficient, and for the latter seldom more than half an hour is allowed.

nient method has been adopted, which consists in passing the calicoes by means of machinery along a set of apparatus known by the name of *steam-chests*. These are large square cast-iron vessels fixed at some little distance from each other, sometimes on an inclined plane, and in other cases perpendicularly one above the other; these are kept constantly full of steam, and are connected with each other by bent pipes either of iron or lead. It is so contrived that the pieces of calico are made to pass over each of these heated chests in contact with their surfaces; and in consequence of the access of atmospheric air, as they pass from one of these chests to the other, they are generally found to have become thoroughly dried by the time they have arrived at the end of the series. A better apparatus has however been lately invented, consisting of an arrangement of cylinders made of tinned iron or copper filled with steam, and round which the calico is made to pass in succession. In this apparatus there is a vane fixed underneath every two of these cylinders, which is moved by means of the steam-engine, and agitates the air so as very much to facilitate the drying process. This mode of drying printed calicoes is so effectual, that although the cloth be wound from a heap at one extremity of the apparatus upon the first cylinder quite in a wet state, it becomes thoroughly dry by the time it has passed over the whole apparatus, and falls in a perfect state on the heap at the other extremity of the apparatus.

If iron-liquor has been employed in printing the goods, it is an excellent practice to keep them for several days exposed to the atmosphere* after their removal from the stove, as the blacks, pompadours, olives, and indeed every other colour prepared with that metal, will increase in intensity; the goods will clean better in the dung-vessels, as will be explained hereafter, and the colours will rise higher and brighter when they come into the copper of bark or madder.

It may be worth an experiment to discover whether the colours containing iron would not be better if they were suffered to be only a *very short time* in the stove, but were hung up instead for several days, exposed to a current of air at the temperature of the atmosphere; as the iron would thus acquire the oxygen slower, and consequently would be fixed more firmly in the fibres of the cloth.

When the pieces have been properly stoved, they are passed, by means of a winch, through water at various temperatures, with a little cow-dung mixed in it. The intention of the dung is to ab-

* The iron in an acetous solution is in the state of the *black* oxide; but by exposure to the air it acquires a further dose of oxygen, and obtains the state of the *red* or peroxide.

sorb and remove that portion of the mordant which is not actually combined with the cloth, and which otherwise might stain the white or unprinted parts.

This part of the business was formerly conducted in a very uncleanly and negligent way; but of late years some printers have incurred a considerable expense in the construction of their dunging machines, with cocks for hot and cold water attached to them, and thermometers to regulate the temperature.

I suspect, however, that the dung of the cow is serviceable in another way besides that of cleansing,* though few printers may be aware of the nature of its operation.—It is acknowledged that madder, cochineal, and some other dyes, produce much better colours on woollen than on cotton cloths, owing to the former being of animal, and the latter of vegetable origin. I presume, therefore, that the dung imparts an *animal* matter† to the fibres of the cotton, and that this animal matter acts as an additional mordant, and thus more powerfully attracts the colouring particles of the dye, than the mordants alone would be capable of doing.

If a piece of calico, prepared with the acetate of alumina be divided into two parts, and the superfluous mordant removed from the one by cow-dung and water, and from the other by water only, at the same temperature, it will be found, on passing the two portions through a decoction of weld or quercitron bark, that the yellow will be much more intense and bright in that which has been submitted to the action of the cow-dung.

The process of DUNGING calicoes is an operation that varies in time from five to forty minutes, according to the style of the work. The pieces are then taken to the river or wheel, to be more effectually washed; and after this they are passed through tepid water, in order that the workman may be assured that every impurity is removed.

His next care is to provide a copper boiler of pure cold water, into which a sufficient quantity of madder is broken, and then a fire is lighted underneath the copper. The calicoes, printed and rinsed as above, are now put into the boiler, and from the time they are immersed, the workman never ceases to turn the winch, so as to pass every part of the goods repeatedly through the liquor, until the whole acquires a boiling heat. Indeed, this operation is

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* To *cleanse* calicoes by immersion in a dung-vessel, may appear to be a strange phrase; but as this is the technical language of the trade, no other term could be employed with propriety.

† Berthollet, who analyzed the dung of the cow, found in it a substance partaking of the nature of animal *bile*.

sometimes continued for ten or fifteen minutes after the bath of madder actually boils, when the pieces are taken out and washed thoroughly. In most of the large calico-works, a considerable improvement in the method of maddering has been adopted. This consists in giving a perpetual motion to the calico by means of the steam-engine. The ends of several pieces are fastened together, and then they are put into constant motion through the madder-vessel, whereby the colours are rendered more uniform ; and one man is enabled to attend to three dyeing-vessels at one time, which is found to be a great economy of labour.

Madder is one of the most valuable drugs we have, for a variety of purposes in dyeing and calico-printing : as it is the agent by which the best and most permanent blacks are produced ; also the finest purples, and every shade of red from a pale pink to a crimson. But perhaps it may not be generally known that this article improves by age. For instance : If a quantity of madder-roots be ground, and then packed tight in a cask, so as to exclude the air, and kept thus for six months, they will dye a much better colour, and go much further, than they would have done had these roots been used as soon as they were ground.

The process which is called MADDERING, has the effect of imparting all the requisite colours to the goods by means of *one* operation, which may be thus explained :

While one mordant precipitates the colouring matter of the madder to a red, another precipitates a different portion of it to a purple, another precipitates it black, and so of every possible shade, from a lilac to a black, and from a pink to a deep red.

If a portion of weld or bark be added to the madder, every shade from a brown to an orange may be produced ; whereas, if weld or bark alone be employed, all colours between a dark olive and a bright lemon can be imparted to the cloth. These changes are all occasioned by the play of chemical affinities, for the knowledge of which we are indebted to the improved state of chemical science.

The only materials which are employed by the calico-printers for the production of fine yellow are the quercitron bark and the weld plant ; the former is the bark of a peculiar kind of oak growing in America, the latter is the produce of our own country. The weld plant (the *reseda luteola* of Linnæus) is for the most part cultivated about Doncaster in Yorkshire. It is found, however, that nothing impoverishes the land so much ; otherwise it produces a very heavy and profitable crop.

Here it may be worth remarking, that whenever it is of consequence to produce the finest yellows or more delicate lemon col-

our, it is necessary to dry the pieces in the open air, as the stove would not fail to injure such colours; for stove-drying has always a tendency to convert a yellow to an orange It is also necessary to be equally careful in the operation of dunging the mordants for these pale yellows; for should this be done at a higher temperature than 96° or 100°, their beauty will certainly be impaired. There is another advantage in this, viz. by dunging at this low temperature, the dyeing may be completed even at 110° or thereabouts, which will give a much livelier colour than where a higher temperature has been employed.

The mordants generally used in calico-printing, are acetate of iron for browns, blacks, lilacs, &c.; and acetate of alumina for all the different shades of reds and yellows.

The best method that I am acquainted with of preparing the acetate of iron for making the common black colour for calico-printing is as follows:—Take the iron liquor of its full strength, or lowered with one or two waters according to the goodness of this mordant in the first instance, and then add about $2\frac{1}{2}$ lbs. of flour to every gallon of the fluid, more or less according to the goodness of the flour and the kind of work for which it may be designed. Having well incorporated the flour with the iron liquor, let the whole remain twelve hours or more. After this it is to be boiled for about half an hour, or till the whole be converted into a paste of due consistence, when it will be fit for use. The materials become better incorporated, and the colour is found to work better, by lying thus long before it is finished by boiling.

Formerly the acetate of iron was made by digesting old iron hoops in sour beer, or in very weak vinegar; but of late years it has chiefly been made with the pyroligneous acid,* the oleaginous impurities of which tend, in some cases, to improve the mordant.

Blacks are also produced by the nitrate of iron† and gallic acid; the mixture is called chemical black. The nitrate of iron is made by dissolving metallic iron in a peculiar kind of aqua-fortis. Common aqua-fortis will not answer for this purpose; for, though it may dissolve the iron with rapidity, part of the

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* If wood be submitted to an intense heat, when [illegible]sed in an iron vessel of any kind with a proper aperture to allow the vapour to pass, this vapour on being condensed forms the acid in question, and is now known to be a kind of impure *vinegar*. The wood in this case is converted into *charcoal*, of which a great deal is prepared by this process, particularly for the formation of gunpowder.

† Nitrate of iron was not applied to calico-printing till within the last fifty years. This discovery formed an important æra in the trade, as it afforded the manufacturer the means of varying his styles of work in a multiplicity of ways and forms, which till then were entirely unknown.

metal is apt very soon to precipitate; which not only weakens the colour, but leaves the remainder so acidulous that there is always a danger of such a preparation injuring the texture of the cloth.

Chemical black is made by mixing a decoction of galls with the nitrate of iron just described; this produces gallate of iron, and probably some tannate of that metal: the disengaged nitrous acid then dissolves the two newly formed salts. The colour called chemical black may therefore more properly be said to be an union of the colouring matter of galls with the peroxide of iron, than a mixture of gallic acid with nitrate of iron. The gallic acid is probably like the pyroligneous, or nothing but the acetous acid holding a peculiar vegetable substance in solution, and either the muriate or the sulphate of iron, if applied to cloth in conjunction with the colouring matter of galls, will in time produce as intense and durable a black as the nitrate; but the difficulty of thickening the solutions of these salts, and the time which is required to convert their base into a peroxide, render them less fit for the calico-printer's purpose.

It is, however, necessary to remark that the black which is formed by this solution of iron, is produced in a different way from blacks in general; for, when common iron liquor is used for this purpose, it is first printed on the calico; and when it has been sufficiently oxidized by exposure to the air, the goods are boiled in a decoction of madder, which renders such parts as had been printed with the acetate of iron an intense black. But the black from nitrate of iron and galls is applied at once to the cloth, and is *not afterwards increased in intensity, or raised, as it is termed, by* dyeing.

The calico-printer by using a black ready formed is thus enabled to mix it with other colours, in cases where by dyeing alone it could not be produced, as in conjunction with yellows and olives, raised by weld or quercitron bark.

The acetate of alumina is prepared by a mixture of the sulphate of alumina and potash with acetate of lead, both in a state of solution; so that, on the theory of double decomposition, sulphate of lead is formed, which precipitates, while the acetate of alumina remains in solution.

Since the demand for this article has been increased on account of the extension of the printing-trade, it has been prepared from the pyroligneous acid, by means of lime and alum. The following is the method:

The pyroligneous acid is first passed through a still, to divest it of a portion of the oil or tar which is always dissolved in it; it is then saturated with lime or whiting; and lastly, the acetate of

lime thus formed, is decomposed by a heated solution of sulphate of alumina and potash. The result of this double decomposition is sulphate of lime, which precipitates, and acetate of alumina, which is drawn from the sediment of the calcareous sulphate, and preserved for use.

And here it may be necessary to caution the manufacturer against a misfortune that may befall him if he be not conversant with the chemical nature of the substances he employs.

Magnesian limestone abounds in Derbyshire and in some of the adjacent counties ; and should a maker of acetate of alumina employ such lime in his process, the article which it would produce would in all probability be entirely unfit for the use of the calico-printer. But I must be more explicit.

In employing the common lime in conjunction with alum, a sulphate of lime will be formed, as mentioned above, and this being nearly an *insoluble* salt, will precipitate. But here, sulphate of magnesia would also be formed, which being a *soluble* salt, would remain in solution, and increase the specific gravity of the liquor, a circumstance which would be very apt to occasion the deception which I am anxious should be avoided. If magnesian limestone be employed, the liquor will appear good by the hydrometer ; but as it will contain more Epsom salt than acetate of alumina, it will be unfit for every purpose for which it was intended.

While speaking of acetate of alumina, I cannot avoid remarking, that the process which has just been described for making this mordant, and which is followed invariable by many of the manufacturers in the North, is extremely improper, on account of the lime which is employed in it, be the lime ever so good, as that earth is very prejudicial to every species of red dye. The proper way of making it, though more expensive, is that which was originally pointed out by Berthollet, and which consists in decomposing sulphate of alumina and potash (common alum) by means of saccharum saturni, or acetate of lead.

In reverting to the remaining processes of the print-work, it must be noticed, that when the goods have passed through the weld or madder copper, they are usually carried to a boiler containing wheat-bran and water, in which they are winched for a considerable time, for the purpose of freeing the white grounds from the stain which they had acquired from the madder or the weld. This process always impairs, in some measure, the intensity of the colours ; but it is a necessary operation, as there is no other mode so convenient for removing the stain which is always given to the white part of the print by the madder, the bark, or the weld, which has been used in dyeing it.

Branning has also the effect of giving a pink hue to all madder reds. But is not generally known what a peculiar richness may be imparted to madder colours, by raising them with a mixture of bran and madder; that is, by mixing a portion of bran with the madder in the first instance. I have myself sometimes produced colours in this way whose brilliancy has astonished me.

This mode of dyeing red with a mixture of bran and madder is called in the trade *Growse's* or London pink, and was invented by a journeyman calico-printer near London, of the name of Growse, who sold the recipe for 100 guineas. In this process the acid of the bran prevents the precipitation of the *fawn-coloured* particles of the madder, and the red produced is consequently more rosy and pure.

Madder contains two distinct species of colouring particles, a beautiful red or rose colour and a degraded yellow or fawn colour —both are precipitated on the cloth, in the usual dyeing process, to the great injury of the *tint* obtained. The object of the tedious Turkey-red process is to fix the red colouring particles so firmly as to allow the fawn-coloured ones to be dissolved and discharged by long boiling in solution of soap or soda, by oxymuriate of soda or by acid solutions of tin. A dilute supersulphate of potash has lately been used with success for this purpose in France.*

It frequently is the case, however, that goods will not bear to be sufficiently branned to clear the whites entirely by that one operation;† such goods, therefore, are partially cleansed in the branning-copper, and are then laid on the grass for some days, until they become perfectly clean and white.

But, within the last few years a new method has been introduced, which consists in immersing the pieces for a certain time in a very weak solution of one of the bleaching salts, such as oxymuriate of potash, soda, or magnesia. This simple process, which effects in a few minutes what would require more than as many days in grass-bleaching, is now much practised, and promises very soon to supercede crofting entirely. This is a most important improvement, as some of the large printers formerly required as much land to spread out their goods upon, as would make a farm of a very considerable size.

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* See "A Collection of Observations on Madder," by Berthollet, *Annales de Chimie*, tom iv. page 102. M. Watt "On the Properties of Zealand Madder." *Ibid.* page 104. Vogler "On dyeing Cotton with Madder." *Ibid.* page 109.

† The temperature at which the operation of branning is performed, is very important. If bark yellows are dyed at 100°, it is customary to bran such goods at 115° or 120°, as it is a principle always to bran at a higher temperature than the goods are dyed at. Madder-work must be branned at a boiling heat.

Besides the kinds of calico-printing already mentioned, there are others which it will be proper to notice in this place. Of these, what is called resist-work, is now done in considerable quantities. It is conducted in the following manner :

A certain preparation of copper, mixed either with flour paste, with gum, or with pipe-clay and gum, is printed on the calico, in any shape or of any pattern that may be desired.* When this is sufficiently dry, the goods are repeatedly dipped in the blue vat till they have acquired that depth of tint which may be required. The art of making an indigo-vat, or blue vat as it is sometimes called, consists in forming such a mixture of lime and sulphate of iron as shall most effectually deoxidize the indigo ; as indigo has no affinity for cloth in its natural or oxygenized state. Hence those parts of a piece which are printed with the solution of copper, will never be dyed blue in one of these vats ; because the deoxidized indigo becomes oxygenated the moment it touches the copper, which parts with its oxygen to the indigo, and occasions it to become insoluble, and consequently incapable of forming a dye.

Thus, while sulphate of iron has the power of deoxidizing indigo, sulphate of copper, or any other salt of that metal, is incapable of retaining its oxygen, whenever it comes in contact with that singular substance in a state of deoxidizement ; and it is a curious instance of the different degree of intensity by which oxygen is held by the different metals.

I suspect, however, there is often great waste in the management of these vats, and that the contents are often thrown away long before the indigo is spent, in consequence of the want of a due admixture of the lime and the sulphate of iron with the colouring matter ; whereas, if a dyer or calico-printer be acquainted with chemical science, he will understand the nature of the operation of these substances upon each other, and hence can be at no loss how to keep up the vat to its original strength, by replenishing it occasionally with one or other of the ingredients as may be required.

The fact is, that indigo, unless it be deoxidized, is insoluble either in the alkalies or the alkaline earths ; but when this is effected by the iron of the copperas, the lime dissolves it, and forms it into a perfect solution of indigo. At first a portion of the lime abstracts the sulphuric acid from the copperas, and precipitates

* The sulphate, the nitrate, the muriate, and the acetate of copper have all been employed for preparing the resist-paste ; but I believe the sulphate is the best for the purpose, unless a very concentrated solution of the four salts were prepared by successively dissolving each of them in pure water.

with it in the state of sulphate of lime. The detached metal being now in the state of the protoxide of iron, and having in that stage of its oxidizement a great affinity for oxygen, it takes it from the indigo, and thus becomes changed to the peroxide of iron, which being insoluble precipitates likewise. Thus the acid and the base of the sulphate of iron are at length both precipitated, when the remaining portion of lime seizes the deoxidized indigo and forms with it the solution desired. When the principle of the blue-vat is thus properly understood, it is an easy thing for the manager, at any time to discover which of the three materials is deficient, and to add it accordingly. I am the more desirous of offering these remarks, because I have many times heard the proprietors of a manufactory declare that they have occasionally thrown away the whole contents of a new vat, containing many pounds worth of indigo, because the foreman or managers of the the works had not observed the due proportions of the lime and the copperas to put the indigo into a proper state for working; and they have an idea, if it is once wrong it must be always so, and therefore they throw it away without attempting to correct the error.

To revert to the process of resist-work, it is necessary to say, that when the pieces have been sufficiently immersed in the blue-vat and have been washed and passed through diluted sulphuric acid, those parts which had been printed with the preparation of copper, are found to be preserved a good white; the preparation having effectually resisted the operation of the indigo, though all the other parts of the cloth have received a permanent dye. The various deep blue calicoes with white spots or white figures, which are now so common, are generally done in this way; and by a similar management with subsequent dyeing in madder, in weld or quercitron bark, figures in red or yellow (as the case may be) are exhibited upon a blue ground.

In some particular styles of work the operation of certain colours is resisted by means of stopping out with wax; but this is too expensive a method to be adopted often, in these times, when it is the object of every manufacturer to finish his prints at the least possible expense. Formerly this mode was very generally practised, and wax was consumed in very large quantities by this process.*

In printing those silk handkerchiefs called Bandanas, a process called waxing is still followed. It consists in making a prepara-

* In the East Indies, wax is still used for preserving the whites in calico-printing.

tion of tallow and rosin very liquid by heat, and in printing it in that state with a block upon the silk. When such goods are passed through the blue-vat, those parts which are covered with the tallow and rosin are preserved from the action of the indigo, and remain white, while all the rest is dyed a fast blue. The method afterwards taken to discharge a part of this blue, and produce yellow, orange, &c. will be mentioned hereafter.

A very singular looking substance was discovered a few years ago near Stockport, which being handed about from one to another, created considerable interest in that neighbourhood. Every body supposing it to be a natural production, specimens of it were sent to a variety of persons in various parts of the kingdom, for their opinion and analysis, and among others a portion was sent to myself. However, after every one had been busily engaged in examination and conjecture, respecting this unknown substance, it was announced, that some seventy or eighty years before, a calico-work had stood on the spot where the article was found, and that this was nothing more than a large heap of the refuse compound of flour, wax, and gum, above mentioned.

The reader will perceive that these *resists* are employed for the purpose of preserving certain parts of a piece white, and of giving other varieties to those goods in which blue is the predominant colour; but if the ground is to be white, and the piece is only to have one small object* in indigo blue, such as a single sprig, then a different management is necessary, and the colour is imparted by a process which is called *pencil-blue*.

Here the indigo is deoxidized by means of orpiment, which is a sulphuret of arsenic; and formerly, whatever objects were done with it were put in by means of a pencil: hence its name, PENCIL-BLUE.

Pencil-blue is composed of the following ingredients, viz. Ten ounces of indigo finely ground in water; twenty ounces of quicklime in lumps; the same quantity of potash of commerce, or the impure sub-carbonate of this alkali; and ten ounces of orpiment. These proportions require one gallon of water, and when the whole has been properly mixed, and time has been allowed for the sediment to subside, the clear colour is to be carefully poured off and thickened with gum senegal.

The intention in using this preparation had always been to impart it to the calico, before the indigo had time to recover its oxygen: hence a small quantity only was mixed at once, or no more than could be pencilled on in a very short time; but the incon-

* *Object* is a technical term belonging to this branch of manufacture.

venience which attended this plan, gave rise at length to an invention which has nearly superseded the use of the pencil.

The invention to which I refer is the spring-sieve, which may be thus described : A fine canvass sieve is suspended within a tub of pencil-blue colour, so contrived that the canvass should be in contact with the colour, and follow it as it is wasted. But as the colour was consumed the sieve did not always exactly sink with it, which occasioned great uncertainty, and sometimes considerable disappointment from the admission of too much atmospheric air :—to obviate these inconveniences many schemes were resorted to, but none of them completely answered the purpose.

This utensil has, however, now been much improved by a particular friend of mine. The improvement consists, essentially, in the addition of a vessel upon the principle of the common bird fountain, which is filled with the prepared indigo and closely corked. This supplies the blue-tub just as fast as it is exhausted.

[*To be continued.*]

ART. XXVII. *On the Barometer.* By J. F. DANIELL, F.R.S. [*Jour. Roy. Inst.*]

THE following paper requires a few words of preface. It contains, as far as I am able to recollect, the substance of a communication which I had the honour to make to the Royal Society, in the month of November last, before the commencement of their last session. As, with a former paper upon the same subject, a mistake had arisen, as I was informed, from my not having expressed a desire to have it read, the consequence of which was that it was not read, I took the precaution of placing the present manuscript in the hands of the President, with a due notification that it was presented for the purpose of being read at the meeting of the society. I went, immediately afterwards, upon the continent ; where I was detained several months. Upon my return, in June, I found that the first part of the *Philosophical Transactions* for the year, had been published, and that it contained no notice of my paper : neither could I find that any communication had been sent to me of its destination. I was naturally anxious to obtain some intelligence respecting it, and addressed a note upon the subject to the President ; to which he returned me the following reply :

Dear Sir, 26, *Park-street, June* 5.

Your paper was read to the Royal Society many months ago,* and has been before the council. There was but one opinion expressed as to the ingenuity of the method of preventing the introduction of air, supposing the *cause* that which you have assigned. The *existence* of this cause for the appearance of elastic matter in barometer tubes, was not considered, however, as proved by the experiments you have detailed, and it was therefore agreed by the council to wait for your return, in hopes that you might be able to give them some new details or elucidations from the researches which you stated were in progress.

I am, dear Sir,

Very sincerely your's,

H. Davy.

I immediately returned the following answer, being anxious to publish the paper in the ensuing number of this Journal; but not acquiescing sufficiently in the reasons assigned by Sir H. Davy to withdraw the paper from the judgment to which I had submitted it.

My dear Sir,

I have no new details or elucidations to communicate to the Royal Society. I shall therefore be much obliged to you to have my paper brought again before the council for their final determination; unlesss indeed I am to understand that their determination has been already expressed.

I have the honour to be, dear Sir,

Very truly your's,

To Sir H. Davy, Bart., J. F. Daniell.
President of the Royal Society, &c.

In consequence of this, the paper was again laid before the council, just before the recess; when, on account of a division of opinion, their determination was postponed to next year. A member of the council then asked leave to withdraw the paper, in order to allow of its publication elsewhere, and stated that I had neglected to preserve a copy of the manuscript. The president, I am told, objected to this, and laid down the law, that the paper having been once taken into consideration by the council, could not be withdrawn. To make sure that no formality was neglected, I immediately addressed the following note to the president:

* It was read, I am informed, on the twentieth January, and several papers which were delivered in after its communication were read before it.

My dear Sir, *Gower-street, 25th June*, 1825.

Understanding that the decision upon my paper is postponed to next year, I beg permission to withdraw it.

I remain, dear Sir,

Your's faithfully,

To Sir H. Davy, Bart., J. F. DANIELL.
President of the Royal Society, &c.

To this application I never received any answer. This is a brief statement of facts, upon which I shall abstain from making any comment. Twice before I have experienced similar treatment from the council of the Royal Society, and twice before I have appealed to the scientific public with success. I shall now endeavour, from memory and such rough notes as I have preserved, to recompose the paper, with a full conviction that the facts narrated are of sufficient importance to call for immediate publication, and satisfied by the opinion of those who are well qualified to judge, that there is sufficient proof of their existence to satisfy all impartial minds that the subject is well worthy of that further investigation which it was my purpose to propose to the Royal Society. A new law has now been promulgated by the President of the society, and every thing henceforward published in the *Philosophical Transactions* must be considered as proved. The council will doubtless immediately drop the notice which has always hitherto been published in the preface to the volumes, that "they do not pretent to answer for the certainty of the facts or propriety of the reasonings contained in the several papers published, which must still rest upon the credit or judgment of their respective authors;" and they will, of course, be careful that this law is administered with impartiality. In such a regulation I, as an humble individual, cannot but acquiesce; but, with many others I imagine, I shall always be found to resist the curtailment of the hitherto-acknowledged right of an author to withdraw a paper, any time previous to the decision of the council upon its publication.

In the year 1823 I presented to the Royal Socicty a paper upon the Construction of the Barometer, the substance of which I afterwards published in my volume of Essays, the original paper having been committed to the archives of that learned body. I therein stated my reasons for differing from the high authority of the President, upon the *cause of the existence of elastic matter in borometer tubes*, suggested to him in a paper upon "the electrical phenomena exhibited in vacuo," and published in the *Philosophical Transactions* for the year 1822. Sign. Bellani also arrived

at the same conclusion as myself, from a series of experiments which he undertook, expressly to determine whether the air or vapour, the last portions of which are found to remain so obstinately in barometers and thermometers, is introduced with the mercury, or is a portion of that which originally occupied the tube before the introduction of the metal. The conclusion he comes to is, that it is always a portion of that which previously adhered to the glass, and *that mercury is utterly incapable of absorbing either air or moisture.* One of his experements is so simple, and at the same time so conclusive, that I cannot refrain from giving a short account of it. He filled a barometer tube, and boiled it very carefully, and then prepared a funnel made of a small capillary tube, which reached through the mercury in the barometer-tube, to the closed end, and was enlarged at the top. When introduced, it had been recently made, and perfectly dry. Some mercury was then prepared by agitating it in a bottle with water and air, and dryed by means of filtering paper, and afterwards passing it through paper cones, three or four times, into dry vessels. A little of this mercury was poured into the funnel tube, and the air extracted by means of a fine wire, so that the column was continuous. So much of this prepared mercury was then poured in as fully to displace the mercury which had been boiled in the tube. The barometer was found to stand exactly at the same height as before in the same circumstances; and, when the mercury was heated, none of those bubbles appeared which arose on the first boiling.*

Still further to illustrate this subject, which I thought of the highest importance, and to ascertain the difference of capillary action in boiled and unboiled barometer-tubes, I undertook the following experiments. The apparatus, which I made use of, consisted of an upright pillar of brass, standing upon a mahogany foot, upon which two horizontal arms of unequal lengths were made to slide; at the extremity of each of these a steel needle, with a fine point, was fixed perpendicularly downwards. These points could be adjusted to the same plane, or their relative distance be measured, by means of a nut and screw upon the pillar, which carried a *nonius;* and the slightest contact of these points with the clean surface of a basin of mercury was instantly perceptible. I satisfind myself, by repeated trials, that the adjustment might be depended upon to the one-thousandth part of an inch. I made a contrivance to hold a glass tube perpendicularly immersed in a basin filled with mercury; and when one of the steel points was made to touch the surface of the fluid in the tube, and

* *Giornale de Fiscica*, Vol. vi. p. 20, or see *Royal Institution Journal*, Vol. xv. p. 371.

the other the surface in the basin, the depression of the former was accurately measured by the *nonius*. In this manner I determined the capillary action of several tubes, varying in their diameters from one to six tenths of an inch. The results agreed as nearly as possible with Dr Young's table, calculated from the experiments of Mr Cavendish. The end of the tubes, opposite to those at which the measures had been taken, were then hermetically sealed, in such a manner as to be readily re-opened under mercury : they were immediately filled with mercury, and carefully boiled. I expected to be able to ascertain the differences of depression by opening them in the basin of mercury, and proceeding as before. The experiment was performed as soon after the operation of boiling, as the mercury in the tube had cooled down to the temperature of that in the basin. At first the attraction between the mercury and the glass appeared to be perfect, and no depression could be perceived. When, however, the tubes were left some time exposed, either before or after they were opened, the air and moisture insinuated themselves between the metal and the glass, and an immediate depression was the consequence. This depression increased gradually, till at length it became fixed at the exact point of that of the unboiled tube. The progress of this effect was easily perceptible with a magnifying glass, and was rendered still more visible by heating the tube, when air-bubbles were immediately detached. This is obviously the same effect as that described by Sir H. Davy in his paper before alluded to, in which he says, that, " on keeping the stop-cock of one of the tubes, used in the experiment on the mercurial vacuum, open for some hours, it was found that the lower stratum of mercury had imbibed air, for when heated in *vacuo* it emitted it distinctly from a space of a quarter of an inch of the column ; smaller quantities were disengaged from the next part of the column, and its production ceased at about an inch high in the tube." Now, I believe that I should not be too presumptuous if, stopping here, I were to maintain that my experiment presented absolute *proof* that the air had insinuated itself between the mercury and the tube, and shewed that there was no " reason to believe that this air existed in mercury in the same invisible state as in water, that is, distributed through its pores." For, if the latter had been the case, the mercury, which contained no air after being boiled, would, from its greater density, have sunk in the tube, when surrounded by mercury which had not been boiled, and would have risen gradually as it became saturated with air. I am justified in drawing the conclusion from the contrary effect, that the air had insinuated itself between the metal and the tube, for

the capillary depression is known to be in inverse proportion to the affinity of the fluid for the containing tube, and nothing could have affected the affinity in the case before us, but the gradual interposition of a thin stratum of air and moisture.

Having thus traced and measured the progress of the air down the sides of small tubes filled with mercury, and boiled with the greatest care, I was naturally led to suspect that the same action might take place in barometers, to their gradual deterioration. I soon saw reason to conclude that such a process actually was going on in the most carefully constructed instruments, and that, in time, air would thus insinuate itself into the best Torricellian vacuum. In the paper upon the construction of the barometer, to which I have before alluded, I gave all the particulars of the making of two barometers, in which every precaution was used to dispel every particle of air. One of these was of very large dimensions, and was fixed up in the apartments of the Royal Society, under the superintendence of the Meteorological Committee. The other was of the mountain construction, and intended for my own use. The agreement between these two instruments, when all corrections were made for the differences in their sizes and forms, was very perfect, and proved that the care which had been bestowed upon them had not been thrown away. In the latter, however, I lately remarked that a small quantity of air had ascended into the *vacuum*. I could not discover any way in which this could have obtained admission; but, attributing it to accident, I laid it aside and thought no more of it till the present experiments recalled it to my recollection. By a singular coincidence I was, about this time, informed that the barometer of the Royal Society had assumed a very remarkable appearance, and that the mercurial column, which was originally perfectly bright and compact, now seemed dull and speckled. I immediately proceeded to examine it carefully, and I at once perceived that it was copiously studded with minute air-bubbles. As far as the mercury was exposed to view, the specks could be traced decreasing in size, from the upper to the lower part. The manner in which this instrument is fixed rendered it impossible to suspect that this air could have obtained admission by any accident; for, unlike the mountain barometer, the column of metal is exposed to no oscillations but such as arise from differences of atmospheric pressure. I was myself quite satisfied, and those who have read the account of the precautions taken in filling the tube will also, I think, be satisfied, that this air was not left at its original construction. I now leave it to the candid to judge, whether "the cause which I have assigned for the appearance of this

elastic matter in barometer tubes has not *already* been proved by the experiments which I have detailed."

While I was thus occupied with these considerations, and sufficiently vexed to find that all my care had been thrown away to prevent my adopting that opinion without very strong grounds, it occurred to me that I had, in the course of my experience, observed a phenomenon, which was calculated to throw some light upon the present question; namely, that gasses were more readily preserved from mixture with atmospheric air over water than over mercury. I was unable to refer to any notes of experiments to confirm this suspicion, but I proposed the question to Mr Faraday, who, I made no doubt, from his great accuracy and experience, must have made the observation, if it were founded in fact. Without at the time having any knowledge of the ulterior object which I had in view, he at once answered me, that mercury, he believed, would not confine gases for a long period so well as water; and he thought, that by referring to his note-book, he could furnish me with the particulars of a case in point. He accordingly did me the favour to extract the following particulars:—

In June, 1823, he made a mixture of one volume of oxygen and two volumes of hydrogen; with this he filled five *dry* bottles over mercury, and also four bottles over water. He left the glasses inverted over mercury and water, placing three mercury and two water bottles in the windows, so as to receive the sun's rays and day-light; and two mercury and two water bottles he placed in a dark place. In July, 1824, he examined the bottles; the water bottle in the light contained hydrogen and some common air, and there was no alteration of volume; the mercury bottle in light contained common air only. The water bottles in the dark place showed no alteration of volume, and the air contained in them proved to be the *original mixture;* the mercury bottles in the same situation contained *nothing but common air.*

Now, if I had not known, from the authority of the President, that the council of the Royal Society did not think this *sufficient proof*, I should very confidently have concluded from these facts, that a fluid which has attraction enough for glass, to enable it to wet its surface, effectually prevents the passage of gasses into or out of vessels of that substance; while a fluid which does not wet the surface permits their slow penetration. I should, moreover, have ventured to affirm, that the case of the confined air is exactly analogous to that of the barometer; for its escape and the admission of the atmospheric air can only be in virtue of the law discovered by Mr Dalton, that the gases are as *vacuo* to one another. The inference is also pretty strong, that the filtration

takes place along the surface of the glass, and not through the pores of the fluid.

It has been attempted, I understand, to controvert this conclusion, by the observation that gases have been preserved a considerable time by mercury; but when it is considered that the slight film of moisture, or any foulness of the mercury, will form a connexion between the metal and the glass, the objection can be of no avail, unless these circumstances have been attended to. To ensure the maximum of the effect which I have been describing, it is necessary that both glass and mercury should be in the driest and cleanest possible state; that is to say, exactly in the state in which they exist in a well made barometer.

That another attempt has been made at explanation I can scarcely credit; namely, that some facetious person had played Mr Faraday a trick. The particulars of the case disprove the possibility of such a circumstance, unless upon the supposition that such person foresaw the present discussion. The character, however, of Mr Faraday for precision, renders it unnecessary to say any more, than that he informs me there is not the slightest ground for such a suspicion.

I was no sooner convinced that the most carefully constructed barometers were liable to a slow and gradual deterioration in the manner which I have indicated, than I endeavoured to find a remedy to the evil; without which, it is clear, that some of the most interesting problems of meteorology must be for ever left in a state of vagueness and uncertainty. For a long time I despaired of success; but when at length I discovered an effectual method of preserving the Torricellian *vacuum*, I flattered myself that the Royal Society would so far have taken an interest in the subject as to have ordered it to be submitted to a trial, which could not have been so satisfactorily made under any other superintendence.

I soon perceived that the only possibility of effecting the object which I had in view, consisted in discovering some method of making the mercury *wet* (if I may be allowed the term) the tube in which it is contained. I was fearful, at first, that all the substances, to which its attraction is sufficiently strong for this purpose, would be so much acted upon as to become disintegrated or dissolved. I, however, fortunately recollected that, in some experiments in which I was formerly engaged, platinum, immersed in boiling mercury, became completely coated by it, and afterwards retained its coating for a long time. I repeated the experiment with some platinum foil, and found it to succeed perfectly. The mercury adhered strongly to the foil, and the latter, after a long

immersion, was found to have lost none of its tenacity. I availed myself of this property in the following way :—I caused a small thin piece of platinum tube to be made about the third of an inch in length, and of the diameter of the glass tube ; this was carefully welded to its open end, so that the barometer tube terminated in a ring of platinum. The tube was filled and boiled as usual, and the infiltration of air was completely prevented by the adhesion of the mercury, both to the interior and exterior surface of the platinum guard. I have no doubt that a mere ring of wire welded, or even cemented upon the exterior surface of the glass, which would be a much easier and less expensive operation, would be a sufficient protection, as the slightest line of perfect contact must effectually arrest the passage of the air : but in the first attempt I was desirous that the experiment should be tried in the most perfect manner. When a piece of glass, armed either with a ring or tube, is immersed in mercury, the effect is easily perceived ; instead of any depression being visible around it, the mercury may be lifted by it considerably above its proper level. Time, of course, will be requisite fully to confirm the efficacy of the guard, and I was in hopes that the Royal Society would have attributed weight enough to the observations which I submitted to them, to induce them to give orders for the construction of a large barometer, upon the principle which I have suggested, to be placed beside the two others already in their possession. An opportunity would thus have been afforded of determining, in the most unexceptionable manner, several facts of the utmost importance to meteorological science. The further experiments which the council have called for, before they would commit themselves by the publication of the foregoing opinions, are in progress, and shall be laid before the society as soon as they are complete ; but they obviously require considerable time for their progress. I have taken the pains to re-write this paper, under the conviction that advantage will be derived to science, by sooner throwing the subject open to general observation and experiment.

I expected to have been able to find some evidence of the deterioration of barometers in the numerous registers that are kept of their oscillations : but I have not discovered any which have been continued for a sufficient length of time, with the same instruments, to answer this purpose satisfactorily. Instances abound of observers having taken the pains to re-boil their barometers, from air having obtained admission, in some unknown way which has always been attributed to accident ; but the fact of their gradual deterioration cannot, in this way, be established so completely as might have been supposed.

The register of the Royal Observatory at Paris has only been published since the year 1816, in the *Annales de Chimie* ; a period which is not sufficient so far to neutralize the annual oscillation as to afford the means of a satisfactory comparison. Mr Howard, however, in his work upon the climate of London, states the mean height of the Royal Society's barometer for ten years, from 1797 to 1806, to be 29.882 inches, while for the ten succeeding years, it is only 29.849 inches, which give a depression of .033 in that interval.

The observations of the following ten years will not, I fear, be available in the same comparison, from the carelessness with which they have been made.

The difference in the height of the old and new barometer, which have now been placed side by side, was, in the latter part of the year 1824, .07 inch, upon a mean of twenty observations ; the new barometer standing upon the average so much higher than the old one. Whether this be wholly owing to detorioration, it is not possible to say ; for the old barometer does not appear to have been boiled : but from the well known accuracy of Mr Cavendish, under whose superintendence it was constructed, it is impossible not to ascribe a great portion of it to this cause. The mercury of this instrument is now thickly studded with air-bubbles of much larger size than those of the new barometer : and when I last examined it, some of them were just upon the point of making their escape.

[To be continued.]

Art. XXVIII. *Mr Children's Summary View of the Atomic Theory according to the Hypothesis adopted by M. Berzelius.* [*Ann. Phil.*]

[Continued from page 193.]

The weight of the atom of any body is easily determined, if we know correctly the composition of one or more of the combinations it is capable of forming with any other body, the weight of whose atom has been previously ascertained. Sulphur, for instance, combines with oxygen in several proportions ; in the lowest, 100 parts of sulphur take 50 of oxygen ; in the next, 100 ; and the third, 150 ;* numbers which are in the ratio of

* There is a fourth compound formed of an atom of sulphurous acid united to an atom of sulphuric acid, and containing 100 sulphur + 125 oxygen. Its atomic composition may be stated as just mentioned, or, as consisting of 2 atoms sulphur + 5 atoms oxygen. It is not necessary to say more about it in this place.

1, 2, 3; we may, therefore, assume that in the different oxydes an atom of sulphur is united successively to 1, 2, and 3 atoms of oxygen, and the supposition is supported by various considerations of the other combinations of sulphur, as, for instance, those of the sulphurous and sulphuric acids. The lowest compound, therefore, may be considered as containing an atom of each element, and if we call that of oxygen 8, we find by a simple proportion that that of the atom of sulphur is 16.*

This example is sufficient to show the method to be adopted in similar researches, and it is evident that when the weight of the atom of any one body is ascertained, it may be employed for determining that of other bodies.

The results of a mineral analysis may be calculated on the atomic theory, and the inevitable small errors of experiment corrected by its means.

Suppose we have found that a sulphuret of lead is composed of

Lead	86
Sulphur	14
	100

Here a certain number of atoms of lead, whose total weight is 86, were combined with a certain number of atoms of sulphur, whose weight is 14. If, therefore, we divide 86 by the number representing the weight of the atom of lead (which we find in the tables is 104), and 14 by that of the atom of sulphur, (16), suppressing the decimal point in both cases, we find that the compound contains 82 atoms of lead and 87 atoms of sulphur, numbers which are very nearly equal. Hence we conclude that the mineral is composed of 1 atom of lead and 1 atom of sulphur; and if we calculate the results which our analysis ought to give on this supposition, we find the numbers to be

Lead	86·66
Sulphur	13·33

which accord very nearly with the results of the experiment.

A similar operation will enable us to find the atomic composition of all other binary compounds, whose analysis is known.

Let us now take an instance of some more complex compounds, and calculate them on the data and numbers assumed by Berzelius.†

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* I adopt the numbers given by Brande and Phillips, in which hydrogen is taken as unity.

† In which oxygen = 100. The examples are taken from Beudant, p. 225, et seq.

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Suppose we have found that a sulphuret of lead is composed of

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	100

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A similar operation will enable us to find the atomic composition of all other binary compounds, whose analysis is known.

Let us now take an instance of some more complex compounds, and calculate them on the data and numbers assumed by Berzelius.†

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* I adopt the numbers given by Brande and Phillips, in which hydrogen is taken as unity.

† In which oxygen = 100. The examples are taken from Beudant, *p.* 225, *et seq.*

Suppose an analysis of molybdate of lead (a ternary combination) had given

Oxide of Lead	61
Molybdic acid	39
	100

We find in the annexed table, that the quantity of oxygen in oxide of lead is 7·171 per cent. and that in molybdic acid 33·45; consequently 61 of the former contain 4·37 of oxygen, and 39 of the latter 13·04; but 4·37 : 13·04 :: 1 : 3; or the oxygen of the acid is three times that of the base; but we observe in the tables that the base contains only 2 atoms of oxygen, whilst the acid contains 3; therefore to preserve the ratio of 1 : 3, there must be 2 atoms of acid to 1 of base. The results of the analysis calculated on these data give

Oxide of lead	60·86
Molybdic acid	39.14
	100·00

Let us next take an analysis of copper pyrites, and suppose that it has given us

Copper	34
Iron	30
Sulphur	36
	100

The atom of copper by the table is 791·39; that of iron 678·43. Therefore $\frac{34}{791·39} = 429$ atoms of copper; $\frac{30}{678·43} = 442$ atoms of iron, and $\frac{36}{201·16} = 1789$ atoms of sulphur. Now these numbers are nearly as 1, 1 and 4, and consequently the sulphur must be equally divided between the two metals, so as to form bisulphurets, each containing 1 atom of metal, and 2 atoms of sulphur. If we calculate the composition of the pyrites according to these numbers, we shall have

Bisulphuret of copper	52·48
Bisulphuret of iron	47·52
	100.00

Or if we take the elements separately,

Copper	34·79
Iron	29·82
Sulphur	35·39
	100·00

which agrees very nearly with the experimental results, and confirms their accuracy.

Let us now take the analysis of a quaternary compound, a variety of emerald, which gave

			Atoms.
Silica	68·64	or oxygen 34·52	= 8
Alumina	17·96	8·38	= 2
Glucina	13·40	4·17	= 1
	100·00		

By the tables, we find the respective quantities of oxygen in the three elements of the mineral as stated above. Now we may consider this compound (says M. Beudant) in two ways, either as consisting of one base (glucina) united to a double acid (silica and alumina), or as a double salt formed of the silicate of alumina and silicate of glucina; both views lead to the same conclusion. In the first case the mineral is supposed to consist of 2 atoms of acid (composed of 4 atoms of silica and 1 atom of alumina) combined with 1 atom of glucina. In the second manner of considering the compounds, the general law proposed by Berzelius requires that the acid of one of the salts should be a multiple by a whole number of the acid of the other, which may happen in different ways, but in consequence of the tendency of glucina to form salts with excess of acid, the most simple mode is to consider the silica as equally divided between the two bases, which gives us a quadrisilicate of glucina and a bisilicate of alumina. The first of these salts contains 4 atoms of silica and 1 atom of glucina, forming 1 atom of quadrisilicate; the second contains 4 atoms of silica, and 2 atoms of alumina, forming 2 atoms of bisilicate, because all the oxides contain the same number of atoms of oxygen. The composition of the mineral on the first supposition is,

Quadrisilicate of alumina	86·28
Glucina	13·72
	100·00

And on the second,

Quadrisilicate of glucina	47·71
Bisilicate of alumina	52·29
	100·00

which are composed of

Silica	33·99
Glucina	13·72
Silica	33·99
Alumina	18·30
	100·00

Or,

Silica	67·98
Glucina	13·72
Alumina	18·30
	100·00

Let us take another example of a quaternary compound as a good specimen of the mode of reasoning adopted in these calculations.

The analysis of zoisite gives

		Ox.	Atoms.
Alumina	33	15·42	2
Silica	43	21·62	3
Lime	24	6·74	1
	100		

In this case the silica must be so divided between the two bases as to form a silicate of alumina containing 2 atoms of silica and 2 atoms of alumina, and a silicate of lime, in which the quantity of the oxygen in the acid is equal to that in the oxide; the oxygen in the first salt is, therefore, double the oxygen in the second. Now lime contains 2 atoms of oxygen, and silica 3 atoms; consequently to preserve the equality of oxygen in the two bodies, there must be 3 atoms of lime and 2 atoms of silica. The total quantity of oxygen in this silicate is, therefore, 12, and that in the silicate of alumina 24; but in that compound there are only 6 atoms of oxygen; therefore the salt must contain 4 atoms of silicate of alumina. According to this, we have

Silicate of alumina	59·47
Silicate of lime	40·53
	100·00

which are composed as follows:

Alumina	30·66
Silica	28·81
Lime	26·13
Silica	14·40

Or,

Alumina	30·66
Lime	26·13
Silica	43·21

Hence if the substance operated on was pure, a small portion of lime has, in the analysis, been confounded with the alumina.

As another example, and one well worthy to follow the preceding, we will take the analysis of a variety of topaz. The results gave

		Ox.	Atoms.
Alumina	59	27·55	5
Silica	34	17·10	3
Fluoric acid	7	5·09	1
	100		

We may consider this mineral either as a compound formed by the combination of a double acid (silica and fluoric acid) with alumina; or as a double salt, consisting of one base united to two different acids; that is, as a fluate, and a silicate of alumina. In this instance, the alumina naturally divides itself into two portions, whose quantities of oxygen are 3 and 2. The first portion is combined with a quantity of silica, containing 3 atoms of oxygen, and forms a silicate; the second is combined with a quantity of fluoric acid, whose oxygen is 1. Hence it follows that the oxygen of the first salt is to the oxygen of the second in the ratio of 6 : 3, or 2 : 1. Now fluoric acid contains 2 atoms of oxygen, and alumina 3; the bi-aluminous fluate must, therefore, be formed of 4 atoms of alumina and 3 atoms of acid, in order to preserve the ratio of 2 : 1; the oxygen of this salt, therefore, is 18; but an atom of silicate of alumina contains only 6 atoms of oxygen, because these two oxides have each 3 atoms; and as the oxygen of the silicate must be double that of the fluate, there must be in this compound 6 atoms of silicate. On these data topaz is formed of

Silicate of alumina	68·70
Bi-aluminous fluate	31·30
	100·00

Or, taking the elements separately,

Silica	33·00
Alumina	35·72
Fluoric acid	7·61
Alumina	23·67
	100·00

Or,

Silica	33·00
Fluoric acid	7·61
Alumina	59·39
	100·00

Or, on the first hypothesis,

Fluo-silicic acid	40·61
Alumina	59·39
	100·00

In his *Nouveau Système Minèralogique*, Berzelius frequently calculates the results of the analyses of minerals consisting of metallic alloys, or sulphurets, from the quantity of oxygen which each ingredient would take if reduced to proportionate degrees of oxidation.

An ore of antimoniated silver, analyzed by Klaproth, gave

Silver	77
Antimony	23
	100

and its atomic constitution is thus calculated by Berzelius,*

Argent	77	prenant oxygene en degrés proportionels d'oxidation.	5.798.	2. 77
Antimoine	23		2.850.	1. 23

Now the weights of the atoms of silver and antimony in Berzelius's table are,

Silver	= 2703
Antimony	= 1613

and the protoxide of silver contains 2 atoms of oxygen, and that of antimony 3, and no lower states of oxidation of either metal are mentioned :

2703 : 200 :: 77 : 5·69
1613 : 300 :: 23 : 4·27

But 5·69 : 4·27 is not in the ratio of 2 : 1 ; to obtain which, and reduce the metals to a *proportionate* degree of oxidation, recourse is had to an imaginary oxide of antimony, at a lower degree of oxidation than the lowest known oxide of that metal ; and it is consequently assumed that the 23 parts of antimony, if reduced to the state of oxide, would require only two-thirds as much oxygen as by the tables they ought to take. Is this good logic ?

We have now to exhibit the method invented by the same philosopher for denoting the composition of chemical compounds by symbols, "in order to facilitate the expression of the proportions of their elements, and to enable us to state briefly and easily the number of elementary atoms which any of them may contain." The nomenclature is wholly taken from the Latin.

1. Simple bodies not metallic are denoted merely by the initial letter of the Latin name of each substance, even though the same letter be common to some of the metals ; thus S = sulphur ; C = carbonicum, P = phosphorus, B = boracium, (boron), &c.

2. A metal whose initial letter is not common to any other elementary body is denoted, like the preceding substances, by

* Nouveau Système, p. 50.

the letter alone, as U = uranium, K = kalium (potassium), L = lithium, &c.; but if the initial be common to another metal, or to either of the simple non-metallic substances, then the two first letters are taken, as Si = silicum, Au = aurum, &c.; but if both of the first and second letters be common to more than one metal, then, instead of the *second* letter of the name, the *first different consonant* is annexed to the initial letter. Thus St = stibium, Sn = stannum, &c.

The chemical sign only denotes a single atom; if it be necessary to express more than one atom, a figure is placed to the left of the sign; thus, Cu + O, denotes oxidulous copper, and Cu + 2 O, oxide of copper; but as this method would be inconveniently long for expressing the composition of a compound of the second order, Berzelius abridges it by omitting the letter O, and denoting the atoms of oxygen by dots placed over the sign of the base, the number of dots indicating that of the atoms of oxygen combined with it. The oxides of copper, for instance, instead of the preceding signs, are indicated by Ċu, and C̈u; the sign of sulphurous acid is S̈, that of sulphuric acid S⃛, and so on; and in the salts of copper Ċu S⃛, = oxidulous sulphate of copper, and C̈u S⃛^{2}, sulphate of copper; the little figure placed above, like an algebraical exponent, indicating that in the latter compound there are 2 atoms of sulphur or sulphuric acid to 1 of base.

The composition of atoms of the third order is denoted after the same manner; for instance, C̈a C̈^{2} + M̈g C̈^{2} represents the mineral called *dolomite*, which is composed of an atom of carbonate of lime, and an atom of carbonate of magnesia. The formula for alum is K̈ S̈^{2} + 2 ⃛AL S⃛^{3} + 48 Aq; and indicates its composition to be 1 atom of sulphate of potash + 2 atoms of sulphate of alumina + 48 atoms of water (aqua). The small exponential figure refers only to the initial sign immediately preceding it; but the coefficient applies to each element contained between the sign +; as, for instance, in the preceding examples, the exponent ^{3} means that 3 atoms of sulphuric acid combine with the atom of alumina to form the sulphate, and the coefficient denotes that 2 atoms of that salt are taken.

So far the symbols are tolerably simple and intelligible; but we frequently meet with such expressions as the following:— ⃛Al S̈i, silicias aluminicus; Äl^2 S̈i, silicias bialuminicus; C̈a^3 S̈i^2, silicias calcicus; C̈a^3 S̈i^4, bisilicias calcicus; ⃛Al S⃛^{3}, sulphas

aluminicus; Äl S̈, sulphas trialuminicus; C̈a S̈², sulphas calcicus, &c. which require a little further explanation.

In order to understand the meaning of these and similar formulæ, it is necessary to state some peculiarities in Berzelius's views with respect to the composition of certain bases.

If we refer to Thomson's or Phillips's table of the weights of atoms, we shall find that lime, baryta, strontita, alumina, magnesia, &c. as well as most of the protoxides of the common metals, as lead, iron, tin, mercury, &c. &c. contain respectively 1 atom of base and 1 atom of oxygen. But Berzelius considers them as containing 2, and some of them 3 atoms of oxygen, for the following reason:—"If we take," he observes, "a comprehensive, general view of the compound bodies that have been correctly analyzed, we find that many of them, particularly the oxides, contain decidedly more than 2 atoms, and that it most frequently is the electro-negative element which enters in a greater proportion than that of a single atom; soda, oxide of lead, carbonic acid, sulphuric acid, &c. may be quoted as familiar instances. This is still more observable in the combinations of compound atoms, as in the salts, where several atoms of the electro-negative oxide are commonly found united to a single atom of the electro-positive. On the other hand there is every reason to believe that the atoms are only united one to one in those bodies which manifest weak affinities, as the gaseous oxide of carbon, the oxidules of copper, mercury, gold, &c.; so that we may presume that all bodies composed of an atom of base and an atom of oxygen have more or less the characters of sub-oxides. It appears, moreover, certain, that the atoms of the stronger acids and bases contain more than one atom of oxygen. Since the number of simple atoms in a compound atom must necessarily influence the form, and consequently the properties of the latter, we have a right to suppose that oxides which contain the same number of atoms of oxygen, have at least some general properties in common which distinguish them from those that have either a greater or a less number. Thus, as we have reason to presume that the oxides which contain only 1 atom of oxygen have the weakest affinities, we find a whole series of more strongly marked salifiable bases, in which the number of the atoms of oxygen must be twice as great as in the former, and therefore it is probable that all the stronger bases contain two atoms of oxygen. Those which contain three atoms of oxygen, on the contrary, are weaker, and many of them

may even be electro-negative with respect to some of the electro-positive oxides;"* that is, act as acids.

Amongst the stronger bases, Berzelius includes all the alkalies and alkaline earths, the protoxides of lead, silver, cadmium, iron, copper, tin, zinc, &c. but alumina, silica, and some others, as may be seen in the table at the end of this abstract, he considers as containing 3 atoms of oxygen. Now, having laid down this arbitrary law, two consequences follow, which the student must keep in mind, or he will be liable to fall into error when he endeavours to translate the formulæ into common language, or compares Berzelius's atomic weights with those of English authors. The first is, that to preserve the proportion between the weight of the oxygen and that of the base, as found by analysis, Berzelius has been obliged to double the weights of the atoms of all those elementary substances whose protoxides he considers as containing two atoms of oxygen, and to treble those which contain three. Thus the protoxide of lead, according to the table (see oxidum plumbicum) contains per cent. 92·83 lead, and 7·171 oxygen, and 7·171 : 92·83 :: 100† : 1294·5; but the weight of the atom of lead in the table is just double that number, viz. 2589. Again the composition per cent. of alumina is aluminum 53·3, oxygen 46·7, and 46·7 : 53·3 :: 100 : 114·11; which is just one-third of the tabular weight of the atom of aluminum, viz. 342·33.‡ Hence the expression $\ddot{C}\,\ddot{\ddot{S}}^3$ means the neutral sulphate of lime, and not the bisulphate which at first view we should probably suppose it to represent, for as the lime contains two hypothetical atoms of oxygen, the salt must also contain two hypothetical atoms of acid, or the canon would be violated which requires that the oxygen of the acid should be a multiple by a whole number of the oxygen of the base. Hence when we find such a symbol as the preceding, and wish to read it correctly, we must remember that all the atoms are doubled, and consequently represented by numbers, which, to reduce them to those of English authors, must be divided by 2. In like manner in the expression $\ddot{\ddot{Al}}\,\ddot{\ddot{S}}^3$ (sulphate of alumina), all the atoms are trebled. As three to one, according to Berzelius's views, represents a neutral salt, so an *equal* number of atoms of acid and base represents a salt *with excess of base;* and accordingly we have $\ddot{\ddot{Al}}\,\ddot{\ddot{S}}$, signifying sulphas trialuminicus.

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* Essai, p. 114, et seq.

† The weight of an atom of oxygen.

‡ To reduce Berzelius's numbers to Thomson's, divide by 100; and to reduce them to Brande's, multiply by $\frac{8}{100}$. Berzelius's number for alumina accords pretty nearly with Thomson's, but not with Brande's or Phillips's. I believe the former to be correct.—C.

We have not yet quite done with these *simple* views; another formula remains to be noticed, namely, that of Äl S̈i. After what we have just shown respecting the sulphas trialuminicus, the reader will probably suppose that this expression means silicias trialuminicus. No such thing; it is the neutral compound, silicias aluminicus. But with respect to this inconsistency, hear Berzelius's own confession.

"I must here point out a little inconsistency which I have committed in the nomenclature of the silicates, by applying the term *silicias* to combinations in which the oxygen of the base is equal to that of the silica. The analogy of silica with acids containing 3 atoms of oxygen would require that this appellation should be given to those compounds in which the oxygen of the silica is three times that of the base. It is evident that these are the true neutral silicates, and that the first are salts with excess of base, since the alkalies, by decomposing a silicate with the assistance of heat, always reduce it to that point at which the silica and the base contain equal quantities of oxygen. However, as the study of the silicates belongs principally to that branch of chemistry which is chiefly applied to mineralogy, and as the nomenclature of the numerous degrees of saturation of silica becomes much more easy by this method, I have thought it right to adopt it.*

Now really if the symbols are intended "to facilitate the expression of the proportions of the elements of chemical compounds, and to enable us to state briefly and *easily* the number of elementary atoms which any of them may contain," we can hardly conceive any contrivance less calculated to answer its purpose! For here in the very same table we have two expressions of precisely the same kind, denoting two very different sorts of compounds, Äl S̈i, representing the neutral silicate of alumina, and Äl S̈ a sulphate of the same base.

Again C̈a^3 S̈i^2 denotes one natural salt, silicate of lime, and C̈a S̈^{2} another equally neutral compound, sulphate of lime.

The formula C̈a^3 S̈i^2 represents, as we have just stated, the scilicate of lime, and C̈a^3 S̈i^4 the bisilicate; the acid of the first containing the same number of atoms as the base, and that of the second twice as many, so that these *brief* and *easy* statements require the reader to multiply the dots over each letter by their respective exponents, and then compare the ratios of the pro-

* Essai sur la Theorie des Proportions Chimiques, p. 170.

ducts before he can tell whether the salt be neutral, super-acid, or with excess of base. This indeed is easy enough in the two instances just mentioned, but it will probably require some reflection before the reader perceives the meaning of $\ddot{F}e^3\ \ddot{S}^4 + 6\ \ddot{F}e\ \ddot{S}^2 + 72$ Aq, and finds out that it means *sulphas biferroso-ferricus cum aquâ.*

[*To be continued.*]

ART. XXIX. *Remarks on Mr Colburn's Theory of Parallel Lines, &c.* By THEODORE STRONG, Professor of Mathematics, &c. in Hamilton College.

I HAVE noticed in the Boston Journal, of October 1825, a New Theory of parallel lines, by Warren Colburn, Esq. which I think inconclusive for the following reasons: *First.* Mr Colburn defines parallel straight lines, to be those which are so situated, as to be equidistant throughout their whole extent. Now it seems to me that Mr Colburn ought to have shown, that this definition is not inconsistent with the definition of a straight line; in other words, that it is possible for two straight lines to be equidistant throughout according to his definition; my views may be illustrated as follows: (see Mr Colburn's Fig. 2d.). Let AB be a straight line, and suppose that CD is another line which is such that the perpendiculars drawn from CD to AB shall always be equal to each other. Now I do not perceive that Mr Colburn has any where shown that CD must, according to this construction, be a straight line. But perhaps I shall be told, that it is so evident as to require no proof. I answer that it appears no more evident to my mind, than "the disputed proposition," which Mr Colburn ultimately endeavours to prove, by the aid of his definition, &c.

Second. Granting, for the sake of argument, that the definition is perfectly unexceptionable. I think that the proof of Mr Colburn's first proposition, in which he undertakes to show that the alternate angles &c. are equal, is inconclusive. For he assumes that the perpendiculars HK, IL, are equal, (see his Fig. 2d.) HK denoting any perpendicular from AB to CD, and IL in like manner, denoting any perpendicular from CD to AB. I do not see how this follows from the definition, for supposing that

the perpendiculars from CD to AB are always equal to each other, how does it appear, that the perpendiculars from AB to CD are also equal to each other? But supposing that these perpendiculars are equal to each other also, where is the proof that the perpendiculars to AB are equal to those drawn to CD. I do not find any satisfactory answer to these questions (nor indeed is there any attempted) in Mr Colburn's paper. I conclude, therefore, that he intended his readers should receive them as axioms. But are they more evident, or indeed so evident, as the "disputed proposition?" Once more. It will perhaps be answered to all that has been said, that the definition means that the perpendiculars to AB shall be equal to the perpendiculars to CD. I answer, if this be the meaning, that the perpendiculars to AB must also be equal to each other, and at the same time the perpendiculars to CD must be equal, for the perpendiculars to AB are each equal to HK, and of course must be equal to each other; also the perpendiculars to CO are each equal to IL and of course to each other; therefore since HK = IL by the definition we must have all the perpendiculars to AB equal to each other, and at the same time the perpendiculars to CD must equal each other; I ask now, where Mr Colburn has exhibited any proof of the possibility of this in his investigations. In conclusion I must observe, that it does not appear to me, that Mr Colburn has in the least degree removed the difficulties attending the theory of parallel straight lines.

I have not made the above remarks from a desire to find fault with Mr Colburn's performances. I can assure him that I have great respect for him as an able mathematician, and a man of science; but I thought that the subject of parallels was of so much importance that his communication ought not to pass without animadversion.

Hamilton College, Clinton, N. Y. Dec. 21, 1825.

Art. XXX.—*On the Diameter of Screws.* By James Dean, A. M. A. A. S.

Every attentive student must have been surprized, on his first examining the investigation of the mechanical powers, at finding that the *diameter* of the screw was not an element in expressing the ratio of the power to the weight necessary to produce motion; and that, other things being equal, the diameter of the cylinder may vary *ad infinitum* without in the least affecting that ratio. That reducing or increasing the diameter to an extreme degree, the distance of the threads remaining the same, will diminish the effect of any given power, will be universally admitted without any very nice reasoning on the sabject. But it seems far from being useless to ascertain the *exact* diameter, which, under any given circumstances, will enable a given power to produce the greatest effect.

For this purpose take the equation given by Bossut, *Traite de Mec.* art. 274.

$$Q = P \times \frac{Cp \times (AB + n \times \text{cir. } Cp)}{CQ \times (\text{cir. } Cp - n \times AB)},$$

wherein P represents the weight acting in the direction of the axis, Q = the power necessary to balance it together with the friction, CQ = its distance from the axis, AB = the distance of the threads taken parallel to the axis; Cp = the radius of the cylinder, and n = the ratio of the friction to the pressure, which, between the same surfaces, is found by Amontons, Parent, Coulomb, &c. to be very nearly constant, and commonly about one third. In order to find the value of Cp, when Q is a minimum, the other quantities remaining constant, P and CQ being omitted, as affecting all the terms equally, let $Cp = x$, $AB = h$, and $\pi = 6{\cdot}28$, &c. and the fraction will become $\frac{hx + n\pi x^2}{\pi x - hn}$, which has its least value when its differential,

$$\frac{h\pi x\,dx + 2n\pi^2 x^2\,dx - h^2 n\,dx - 2hn^2\pi x\,dx - h\pi x\,dx - n\pi^2 x^2\,dx}{\overline{\pi x - hn}^2}$$

$= o$, whence $\pi^2 x^2 - 2hn\pi x = h^2$, and πx (the circumference of the screw) $= h \times (n + \sqrt{1 + n^2})$, whence the diameter may be computed as usual.

From Bossut's formula it appears that when $hn = \pi x$, that is, when the circumference of the screw : the distance of the threads : : the friction : the pressure, the denominator becomes o, and

therefore the power Q must become infinite, or the effect o. When this is the case, if h, or the perpendicular height of the inclined plane, formed by one revolution of the thread be taken $= 1$ or radius, then $n =$ the circumference of the cylinder, or base of the plane, or cotangent of the angle contained between any thread and a plane, perpendicular to the axis. Let this angle be called a, then $n = \text{cot. } a = \frac{\text{cot.}^2 \frac{1}{2} a - 1}{2 \cdot \text{cot. } \frac{1}{2} a}$, by an. trig. whence $\text{cot.}^2 \frac{1}{2} a - 1 = 2n \cdot \text{cot. } \frac{1}{2} a$, and $\text{cot. } \frac{1}{2} a = n + \sqrt{1 + n^2}$, which is precisely the value of the circumference given by the differential calculus when the effect is greatest, so that the angle of inclination at which the greatest effect is produced is exactly half of that angle which, with the same degree of friction, is the limit of all effect.

It hence appears that screws are commonly made much too large in proportion to the distance of the threads, to produce any given effect with the least power. Thus, suppose it be desired to produce a pressure equal to ten tons with a screw whose threads are one inch asunder, lever eight feet long, and friction one third the pressure; if the screw be six inches in diameter, the power must exceed 275lb.; if the diameter be only two inches, any power greater than 122lb. will be sufficient. If the friction amount to half the pressure, the dimensions still remaining unaltered, the powers corresponding to the same two diameters will be 398 and 168lb. respectively. The least powers with these two degrees of friction would be 72lb. and 98lb., and require diameters only 0·4416 in. and 0·515 in. respectively, but no substance known, when reduced to even twice these dimensions, would have strength enough to resist the torsion. It appears therefore that a screw, as an engine of force, with any given distance of thread, ought to be made as small as the strength of the material will admit. In order to ascertain by computation the middle course between wasting materials, labor, and power on one hand, and hazarding the failure of the machine on the other, we may adopt the conclusion arrived at in the En. Br. & Ed. En. "that in resisting torsion the whole lateral cohesion is exerted at $\frac{1}{4}$ the radius of the cylinder from the centre." The notation of Bossut's formula being retained, and S being put for the lateral cohesion of a square unit of the substance used, the area of a section is $\frac{\pi x^2}{2}$, and its strength $\frac{S \pi x^2}{2}$, then $CQ : \frac{x}{4} :: \frac{S \pi x^2}{2} : \frac{S \pi x^3}{8 CQ} =$ the force of the cohesion at

the end of the lever, which being put $= Q = \frac{P}{CQ} \times \frac{h\,x + n\,\pi\,x^2}{\pi\,x - h\,n}$,

and the equation reduced gives $x^3 - \frac{h\,n}{\pi}\,x^2 - \frac{8\,P\,n}{S\,\pi}\,x = \frac{8\,P\,h}{S\,\pi^2}$,

in which if the known quantities, including P and S, are properly substituted, the value of x may be safely adopted for the radius of the cylinder.

But when the exertion of great force or the weakness of the material renders a large cylinder indispensable, the distance of the threads should be greater also, because in that case increasing the distance of the threads does not require by any means a proportional increase of power. Thus with the six inch cylinder abovementioned with the friction of one third, increasing the distance of the threads from one to two inches requires an increase of power less than one sixth, viz. from 275lb. to 319 ; and with a friction of one half, the increase is only one eighth, or from 398 to 448, which may generally be compensated by using a longer lever while the action will be much more prompt and rapid. And less than double the power will produce the same effect, though the distance of the threads be made equal to the diameter of the screw, or six times their former distance. The reason of this will be obvious, when it is considered that in this case the whole power except 37lb is employed in overcoming the friction, and that at such a small angle of elevation the pressure, and consequently the friction, is but slightly varied by the variation of that angle. And with the same distance of the threads, if the diameter receive successive increments the ratios of the corresponding powers will continually approximate to the corresponding ratios of the several diameters, because the effect of the friction, which absorbs most of the power, is proportional to its distance from the fulcrum.

But after all it is far from being eligible in practice always to give screws the shape above described, especially in the fastening of instruments and machines where the object is not to produce but to prevent motion, because if the distance of the threads is greater than n times the circumference, the friction of the screw will not prevent it from running back. On this account it may not be improper to investigate the figure of a screw so formed that the friction shall always hold it where the power leaves it. Let it be required therefore so to shape a screw that, with any given degree of friction, the power necessary to turn it forward shall have a ratio to the power necessary to turn it back, equal to the ratio of the two given quantities $f : b$, f being of course larger

than b. As the length of the lever to which these powers are applied cannot affect their ratio to each other, they may be considered as applied at the surface of the cylinder, and this surface may be further considered as extended on a vertical plane in which case one revolution of the thread is the length of an inclined plane, the circumference of the cylinder is its base, and the distance of the threads its height. Bossut, in art. 273, gives the force which acting parallel to the base (as it always does in the screw) just balances the relative gravity and the friction $= \frac{P \times (HI + n \times GI)}{GI - n \times HI}$, where $P =$ weight, $GI =$ base, $HI =$ height, and $n =$ the ratio of the friction to the pressure. But in order to express the greatest force which, acting in the opposite direction, would not drag the body down the plane, HI in the preceding equation in Bossut, from which the above expression is derived, must be taken — because the force PC or $P \times \frac{HI}{GH}$ or relative gravity, instead of opposing the power, assists it, and the force PM or $Q \times \frac{HI}{GH}$ reversed instead of increasing, now diminishes the pressure which produces the friction or resisting force. With this change the greatest force which (acting parallel to the base and from the summit towards the foot of the plane) will not drag the body down the plane, will be found $= \frac{P \times (-HI + n \times GI)}{GI + n \times HI}$. Let now $HI =$ rad. $= 1$, $n =$ cot. a, as before, and $GI =$ cot. z, z being the angle at the base of the inclined plane, on which, with the given degree of friction, n, the forces forward and backward will have the required ratio $f : b$, and dividing by P, we shall have

$$f : b :: \frac{\text{cot. } a \text{ . cot. } z + 1}{\text{cot. } z - \text{cot. } a} : \frac{\text{cot. } a \text{ . cot. } z - 1}{\text{cot. } z + \text{cot. } a}$$

$::$ cot. $(a - z)$: cot. $(a + z)$ $::$ sin. $2a +$ sin. $2z$: sin. $2a -$ sin. $2z$. Then $f + b : f - b ::$ sin. $2a$: sin. $2z$, whence the base GI or circumference of the screw, and thence its diameter, may be easily found.

By the diameter of the screw or cylinder is always meant the arithmetical mean between the greatest and least diameters of the bearing surface of the thread. The screw is also supposed to have a square thread, or at least that the section of its bearing surface with a plane passing through the axis is a right line and perpendicular to the axis. But when the thread is in the form

of a triangular prism, or rather when the section above mentioned makes an oblique angle with the axis, the pressure and of course the friction is increased in the ratio of the sine of that angle to radius, so that $n \div$ its sine must be taken instead of n through the whole investigation. It is also supposed that there is no pressure except on the threads of the screw; in practice however there is almost always a pressure at the other extremity equal to the weight raised or effect produced, and the increase of power required by the friction resulting from this pressure is proportional to the mean distance of its action from the axis of the screw. The introduction of this consideration would produce conclusions differing still more from the common practice than those drawn in the fourth and fifth paragraphs. In most of the instruments called vices this pressure is distributed over a circular space surrounding the body of the screw, and the friction, thus remote from the axis must absorb a considerable portion of the power; but in the presses, it is commonly confined to the extremity of a pivot, and its retarding effect must be comparatively trifling.

ART. XXXI.—*On Barometrical Observations in relation to the Agitation of the Atmosphere and on the Temperature of Washington.* By JULIUS DE WALLENSTEIN. Read on the 28th January, 1826, before a Scientific Institution at Washington.

Mr President and Gentlemen,

AFTER a steady and scrupulous attention to the various phenomena of the atmosphere, the votary of natural science is irresistibly seized with a desire of drawing, from persevering and minute observations, some general result that may contribute to the advancement of science. But Meteorology is yet very far from giving sufficient data for more than a plausible theory of even a single one of the numerous departments of physics to which it extends. After many years of close application in investigating the variations of temperature, all that can be affirmed is, that in the course of several years, the irregularities compensate themselves in such a way, that the sudden and prodigious increase of heat or cold, for a greater or less number of weeks, does not affect the degree of temperature, which nature

seems to have allotted to every portion of the earth. But whatever attention the generality of observers may have paid to the changes of the atmosphere, probably very few have fulfilled all the conditions, which, in the progress of their investigations, they discovered to be indispensable for obtaining satisfactory results. One may have forgotten to examine the construction of his thermometer, to compare it with a standard of unequivocal precision, or to go through the minute process pointed out by experience and theory, for ascertaining the regularity of its indications. Another may have neglected to bring the instrument into the current of the prevailing wind. The hygrometer requires a frequent repetition of the elementary experiments, by which its scale was determined. The instrument, invented by Saussure, is generally very troublesome, and becomes useless in strong agitations of the air. The greatest caution is necessary when the hair is to be cleansed, and this operation must be frequently repeated. A barometer, that delicate and vexatious companion in a journey, is, upon the whole, the least troublesome instrument during a tranquil residence.

When thus, after a long series of meteorological observations, the various results crowd themselves, in separate columns, under the eye of the examiner, he is, at once, discouraged by the uncertainty of his operations and the disproportion between their utility and the labour they have cost.

Division of labour, which has become so productive of beneficial results in many branches of industry, and is nothing but method and classification, presents its advantages also in science. If in the actual state of Meteorology, it is almost impossible to combine, into one great result, the various modifications observed in the atmosphere, with the instruments adapted to the examination of its constituent parts, and their changes; let us, by dividing the task, endeavour to remove some of the difficulties that present themselves.

The theory of winds is one of the least perfect parts of natural philosophy. Even their course or direction and their velocity have yet been but little attended to. The causes that influence them are connected with facts, the investigation of which constitutes the highest branches of physico-methematical sciences. Without taking into consideration the action of gravitation, the rotation of the earth, the specific weight of the fluids, and the changes which caloric produces on them, the phenomena of the Trade wind could never be explained; the dessicative power of certain winds would be a source of error or a mystery, if the nature of caloric was not understood; and without an intimacy

with the constituent parts, the properties and the effects of the fluid that surrounds us, and, at least a general knowledge of the different formations of our globe, we could not conceive of the origin of vapours, rain, fogs, snow, &c. that succeed each other, sometimes so rapidly, around us, nor explain subterraneous springs and rivers, and by them again, in part, some of the atmospherical phenomena. The sun and the moon act doubtless on the atmosphere. Their combined action is visible and can be rigorously calculated, as far as it extends on the ocean, and the adjacent land, in bays and narrow channels. It is true that the opinion of Galen with respect to the influence of the lunar phases on the crisis of certain distempers, is now disregarded. Able physicians of our day fear only the effect which such opinions could have on patients whose delicate nervous system disposes them to receive every impression that strongly excites their feelings. Olbers, the great astronomer and physician, never thought himself justified to ascribe any influence to the moon on epileptic and nervous disorders and other serious distempers. Yet he does not deny that the relative situation of the sun and moon may have some influence on a few rare maladies.

Returning to the subject, whose obscurities I have presumed to excuse, by mentioning those which cover many other branches of science, and by referring to various examples of their branching out and their real concatenation, I wish to state that in my humble opinion, an assiduous observation of the phenomena connected with the variation of winds, seems to me not one of the least interesting branches of natural philosophy. If the causes by which these perturbations of the air are produced, are yet too little known to throw light on the influence they exercise on all that bears the principle of life and seminal faculties, on our activity, our comfort, and our health, the effects require so much the more attention.

Besides in this department of meteorology, a hope may be indulged that the observations may lead to a series of facts somewhat regularly connected with another ramification of the science. If a great number of observations should show a simultaneous increase or diminution of the height of the barometer, if, during the same current of air, most commonly the mercury of that instrument has varied but little or not at all, if a considerable variation in its range had been followed by a corresponding decrease; these phenomena would deserve the attention of a philosopher, besides their possible reference to good or bad weather, as a means of elucidating hereafter, more and more, the relative dependance of the operations of nature, and of coun-

teracting the errors and illusions, to which we are liable by the imperfections of our organs, our education, and our passions. The prevailing winds have, to be sure, been noted by almost every body that has kept a meteorological journal. In separating from any one of those tables the barometrical observations, in connexion with the corresponding remarks on the state of the heavens, one could bring under a single point of view the possible relation of the pressure of the atmosphere, with its agitations. But in experimental philosophy, facts are only well ascertained, when the observations or experiments are made with an express and exclusive purpose. Surely the temperature is a *function* in every calculation of barometrical changes, but the pressure of the atmosphere is also a *function* in that of the effects of caloric. But like the ramifications of any other department of natural science, they must be separately examined and separately described, although they are afterwards reconsidered in combination with other phenomena.

The method I have adopted, in the annexed table, is withal, in one point of view, but a mean of observing how often certain winds have prevailed in a given time. Should observations be continued with that stated purpose, the view of the subject taken up by the great mathematician Lambert, in the Tranactions of the Academy of Science of Berlin, for 1777, may be brought to a greater certainty, and a formula for determining the force and frequency of given winds, may not be considered much longer as merely an ingenious speculation.

M. de Buch, the scientific friend of Alexander de Humboldt, and the author of a very instructive tour into Lapland, and of many valuable disquisitions, has examined the subject under consideration, in a Memoir addressed to the Berlin Academy in 1818. Of the southwest winds, he observes, that taking the case of their prevalence during three days, one was accompanied with rain. Of 17 days of the northeast wind, one only was misty. The winds from northwest to south, were generally more accompanied with fogs and mist than any other winds. During two days and an half of southwest wind, one was rainy or snowy, and, on the contrary, of 8 days easterly wind, seven were remarkable for dryness. He gives the following table to show the comparative height of the barometer, under the influence of the 8 winds, from north round south, to northeast.

N.	NW.	W.	SW.	S.	SE.	E.	NE.
Lines. 336·32	335·85	335·13	333·61	333·06	334·55	336·36	336·62

In comparing this table with the following, it will be seen, that under the same winds, the barometer is less elevated when it snows.

Lines.							
33325	334·37	333·62	331·93	330·76	332·21	333·38	333·75

The difference of these two tables is as follows:

3·07	1·48	1·51	1·68	2·30	2·34	2·98	2·87

By a prevalence of the north, northeast, and east winds, the difference amounts sometimes to three lines, and the barometer is then as high as it is, on an average, when the wind is from the south. However, snow is more common with north winds, but they operate only, when the snow has already begun to fall; the snow itself owing most probably its existence to an encounter of the fresh air of the north with a misty one from the south. This is exemplified by the following observations quoted in M. de Buch's Memoir.

					Barometer. *Lines.*	Wind.	
1783	March	2,	at 10	o'clock	328·11	SSW.	
"	"	3,	7	"	330·50	NNE.	snow.
"	"	"	2	"	332·7	N.	
"	"	"	10	"	334·8	N.	

When the northern wind becomes steady, the snow falls in small flakes. During the winter, the greatest elevation and depression of the barometer are separated only by a short interval of days, and M. de Buch has observed that the mercury rises more rapidly than it falls. On the 12th March 1786, it snowed with a south-southwest wind, the barometer was at 333 lines, and the thermometer between — 1 and + 1°. M. de Buch explains these phenomena by saying, that the SSW. which kept the thermometer at so low a degree, prevailed in the upper region of the atmosphere, while nearer to the earth, a northern wind exercised its influence. In the upper regions the vapour was transformed into rain, which in its fall turned into snow under the influence of the wind which then prevailed. Effects of contrary winds, in the upper and lower regions of the atmosphere, are the most strikingly observed in mountainous countries. The snow melts often at an elevation of 3000 feet, on the declivity of a mountain, while in the valley the ground is frozen.

The author, whose observations are here quoted, has remarked, that in the places where he has made his investigations, the mean temperature is never lower than — 4·9° of Reaumur (21° Fahr.) when snow has long continued; and the only varia-

tion he mentions is a fall of snow on the 14th of January 1808, somewhere in Norway, under a temperature of — 10·3° (8·8° Fahr.) during an easterly wind. The snow was dry and in small flakes.

I beg to observe, that these citations are, as I find them among my notes; and I regret not having it in my power to lay before you M. de Buch's valuable Memoir, rather than so scanty a specimen as I have presented of what seems to me a fine example of philosophical analysis.

Before I present to you, Gentlemen, my own observations on the climate, where we live together, you will, I hope, permit me to call your attention on the investigations made by a traveller, whose labours are known to the world, who first has described that cluster of islands, the name of which is dear to your heart; who has conducted an expedition round the world with the energy of Cook, the accuracy of Vancouver, and a philosophical mind, not unlike that of Banks and Foster; and whom I, an humble individual, am proud to call my friend. Amongst the researches of this scientific circumnavigator and hydrographer, those on the direction of winds may not yet be known to you, Gentlemen, for I know only of two copies of the valuable work, that contains them, having reached this country a short time since, and where Commodore Krusenstern has, for the first time, supplied the deficiency left in that branch of natural philosophy by Daprés, Dalrymple, Romme, and Horsburgh.

Commodore Krusenstern presents, in the introduction to his Hydrographical Memoirs, accompanying his Atlas of the Pacific ocean, the following outline of a treatise on the winds and currents—"In the Pacific ocean from latitude 30° to the pole the "variable winds" are generally from the northwest and southwest. In the straits of Behring, the north and south winds are the most general. On the coast of Kamschatka, the prevailing winds, from June until the middle of September, are from southwest and west; and the journals of La Pérouse and other navigators prove, that Romme erroneously indicated those of of east and southeast as the most frequent, in the interval above mentioned. On the Kourili islands, according to Capt. Golownin, (whose firmness is likely to be more known to you than his inquisitive mind,) the weather is foggy, when the wind blows from south-southeast to south-southwest, but so dry that the sun is visible. The east and northeast winds are accompanied with rain and storms. The north-northeast and north-northwest bring a clear sky and cold weather. Commodore Krusenstern found on the 31st March, 1805, his thermometer (Reaumur) at $1\frac{1}{2}$°

below zero (28·7° Fahr.) during a wind of north-northeast, which was accompanied with snow. In winds from north-northwest to west-southwest, the weather is cold and the air dry and foggy; and during those from south-southwest to west-southwest, the skies are clear and but seldom clouded, and the temperature remarkably high.

"In September and October, Commodore Krusenstern found on the Oriental coast of Japan variable winds, but more frequently those from the north than the south. On the east and northeast coast of China and on those of Corea, and in the gulf of Tartary, the north and northeast winds blow during nine or ten months, and they turn seldom to the south. In the sea of Sachalin, the winds vary from south to north, according to observations made during 8 years at Ochotzk. With respect to the sea of Ochotzk or the northern portion of the Sachalin sea, the prevailing winds in June, July, and August, are south and east, not strong, and commonly accompanied with fogs and rain. In September, at the setting sun, the wind goes from the south to the west and northwest, blowing pretty strongly from the latter direction during the night. Towards 10 o'clock in the morning, it blows milder and turns to the south. During the whole month of September, the skies are cloudless. From the middle of October till the beginning of December, storms and strong winds are almost uninterrupted from the east or southeast, and the fine weather begins again, when it blows from the north or northwest. From December until March the winds are constantly north and northeast and the weather is fine. In April and May they blow moderately from the north during the night, and from the south during the day. Consequently, the easterly winds are almost permanent in the sea of Ochotzk.

"On the northwest coast of America, from the straits of Behring to 30° of northern latitude, the winds are variable, though M. de Humboldt presumes that from 65° to 50° they blow half a year from the south-southwest and southeast, and the other half from the north and northeast. The southeast wind is there in every season accompanied with rainy and foggy weather, and the northwest with cold and dryness.

"Capt. Cook found in March, in the 44th degree of latitude, a fresh and constant northwest, which continued until the beginning of the summer, with the exception of a southeast, which lasted, however, only 6 hours, and La Perouse, Portlock, and Dixon, did not experience the south winds in the summer. According to Vancouver and the Spanish navigators, the north and northwest are the most prevailing. Hence the difficulty

which is experienced on that coast for advancing to the north, and which can only be remedied by profiting of the east and southeast land breezes, which are of longer duration and stronger, than the sea breezes. Vancouver states that north of Cape Mendocino, the weather is generally bad, and that south of that promontory, the winds are moderate and the skies clear, and that during the winter, the south winds are moderate.

"In Nova Archangelok (57° 2′ latitude N.) the prevailing winds, during the winter, are from the east, accompanied with rain and snow. At the beginning of December gales and storms are frequent.

"On the coast of California, the northwest wind prevails during the summer until October. In November it changes into southwest and southeast with rain, and the weather clears off by a northwest. In October a hot northeast, that prevails for a short time, produces cough and pleurisies. In December and January the mountains begin to be covered with snow, but only for a few hours. In March and April, the winds become variable. During summer and winter thick fogs are frequent. Commodore Krusenstern proceeds with the same minute attention, describing what he calls the 2d and 3d zone, or the portions of the great ocean which lie between the 30th degree N. latitude and 30th degree S. latitude. The trade winds, which prevail in that space, are influenced by the direction of the adjacent coasts, as for instance on those of Peru and Chili, where they are north and south, in consequence of the coasts running in that direction."

But, Gentlemen, it is not to my purpose to follow my scientific friend further in that part of his investigations, howsoever interesting they may be in relation to a different object. I will only add, that the facilities which the constancy of the southerly winds seem to afford for the navigation towards the north, on the last mentioned coasts, are counteracted by currents which drive to the south, and that farther to the north a similar current carries to the west. Between the 30th degree latitude S. and Cape Horn, the regular winds from north and south begin again. Near Coquimbo (30° lat.), the north and northwest winds blow during two or three months in the course of a year, and near Valparaiso (33° lat.) the same winds prevail from May to September, accompanied with rain and fog, but not violent; and during the rest of the year the weather is dry, the winds blowing constantly and strongly from the south. Near Cape Horn the winds that blow most constantly are from the southwest and northwest. Near Cape Horn and in the Sachalin sea, Com-

modore Krusenstern observed a fall of the mercury of nearly 9 lines, though the state of the atmosphere was not altered, and the barometer remained stationary until his departure from those waters, and then the barometer rose again.

According to the manner in which I consider the subject, I humbly believe, that in order to remove the uncertainties which surround it, the principal cause of winds must before all be clearly ascertained, and as soon as we are satisfied of its real nature, the irregularities of its influences must be gathered in as great a number as possible. Hence in a series of remarks on the currents of air prevailing the most in this country, a glance may, and perhaps must, be thrown on the corresponding phenomena under different latitudes and distant meridians.

Winds are the effects of an alteration in the specific weight and elasticity of the air; an alteration, however, which does not pervade equally all its parts. Amongst the latter, we must particularly take in account the atoms, the extremely small particles of water which fill the atmosphere and become only distinctly visible to our eye, when their increased density puts them under the influence of the general action of gravitation. According to these premises, the currents of air, are, by their nature, incessantly changing. Durable winds are, then, but exceptions. They are produced by the difference of the increase of heat in two terrestrial points. The air the most heated, is the most dilated, and its specific weight is diminished; but the circumambient air of less temperature, would take the place of that which has comparatively a greater specific weight, until this air becomes, like the former, dilated by the action of heat. On the contrary, when a portion of the atmosphere becomes colder, its dilatation is diminished, and a warmer air takes the vacant place. This explains the reflux of southern air to the north, at the beginning of autumn or the cold season.

Winds coming from the summits of high mountains are always cold. Even in Guinea, the northwest wind, which blows from the mountains of the interior, is cold and dry. Nevertheless the summits act, by affinity on the product of the evaporation of the sea, and arrest clouds. But the abundance of humidity, which hence results, operates only as a generative cause of water spouts and rivers, according to the geological character of the mountains. Conflicting winds first occasion calms, and afterward storms and tempests. The clouds of different species of electricity, which they possess, produce thunder, the cessation of which is generally accompanied with a change of wind. There are further exceptions, as, for instance, the winds which are

peculiar to certain terrestrial regions, as the Samiel wind of Egypt, Persia, Arabia, and Syria, the north wind of the Gulf of Mexico, the hurricanes of the Cape of Good Hope, and the "Typhon" of the seas of Japan and China, all which have peculiar symptoms, unequal though prodigious strength, and are even distinguished, it seems, by colours. Periodical and trade winds are in part explained by what has been said on the general cause of the currents of air. A more complete *explanation* of these and of tropical winds, as far as plausible speculation may be so called, would require more time than I can command.

In the middle latitudes between the poles and the equator, the winds change oftener than under the tropics. In fine, several winds blow very frequently at the same time in successive regions of the atmosphere. While a strong current agitates the air of a mountain, the inhabitant of the valley enjoys a calm air, and "vice versa." Winds, chiefly those which are dry, accelerate evaporation. This can be explained by their uninterrupted contact with the waters, or the interstices which, under the influence of the elasticity of caloric, they present to vapours, or by the agitation they produce in the liquid atoms. However, an agitated atmosphere is more desiccative than a tranquil air. The vapours, as soon as formed, are carried away, and the air itself becomes not saturated with humidity and does not lose its dissolving force. Saussure remarks that the ascension of fogs and clouds which are composed of vapours is coeval with the rise of the barometer, in consequence of the increased density of the air near the earth. The moist vapours seek then in a region more elevated, an air which is more in equilibrium with them. According to that great philosopher, the smoke and the vapours which surround the summits of Vesuvius and Ætna, rise and fall contemporarily with the mercury of the instrument that indicates the pressure of the atmosphere, and these movements are even considered as prophetic indications. The influence of winds on the temperature extends itself to a greater height than the influence of the direct rays of the sun. The causes which influence the barometer begin to act on the uppermost regions of the atmosphere. In a great part of Europe, the west winds are generally humid and the easterly, on the contrary, very dry. In Switzerland the south and southwest winds are warm and bring commonly rain, and the dryest wind is that from the northeast. The coldest wind in Europe comes from the north. Muschenbroek (according to a quotation of Saussure) has found that in Belgium, the easterly winds are dry, the sky being clear, but when they follow westerly winds, they are accompanied for a

while with rain. In northern Europe winds which tend to turn from north to northeast, pass before through all the other points of the compass; and those which tend to turn from north to west, accomplish it without any deviation.

Let us now collect some facts on the relation of barometrical variations with winds. The source from which I derive them, is again the work, already often quoted, of the great naturalist of the Alps. Generally an increase of heat produces a fall in the barometer. The mercury of that instrument stands higher during the night than the day, in consequence of the diminution of heat. The variations of the barometer are greater in winter* than during the summer in the northern climates, in consequence of the considerable difference of the temperature of the divers regions of the atmosphere, and the proximity of cold and warm air. Local causes produce sometimes rapid and irregular movements of the barometer. When the atmosphere is cooled a few degrees by rain, the barometer rises suddenly, says Saussure, half a line and even a line, without the co-operation of any general cause, and this because a single column of air being condensed by the cold, the other columns which have not undergone that process, extend themselves over it and increase its bulk. The greatest variation of the barometer happens within the interval of a few hours in places distant more than 300 miles. Saussure ascribes this phenomenon to the prodigious velocity of the winds in the upper regions of the atmosphere, where, as it has been already observed, the causes of the barometrical changes are incipient. I will conclude these summary remarks with an observation of Saussure on the exactness of barometrical predictions with respect to rain. In that view, he recapitulates and compares the results presented by several other great naturalists. "The Marquis Voleni has found," says he, "that of 1175 times that, in the course of 12 years, it had rained in Vadua, only 758 produced a fall of the barometer; consequetly of 1000 observations only 645 proved true, and the influence of rain on the barometer seems not great. Van Swinder had collected as many false as true predictions of that nature, but in calculating the observations of one year, according to Mr Horsley's method, he has demonstrated the influence of rain on the barometer, and discovered that in 1778 and 1779 the mean height of that instrument, during *rainy weather*, was less than the *mean height of the whole year*.

* From April 1823 to April 1824, I have found that the height of the barometer was, during the summer months, 29·910 inches, and during the rest of the year 29·926 inches. Difference 0·312 lines.

I now beg leave, Gentlemen, to inform you of some of the results of my own investigations with respect to the apparent connexion between the barometrical variations and the reigning winds.

The extensive table I submit to your examination contains observations made during the months of September, October, and November last, and calculated according to the method and the formula, mentioned in the Memoir, which the Philosophical Society has done me the honour to publish in the last volume of its Transactions.* The following recapitulation brings to a still clearer and more comprehensive view, the various results which can be deduced from those observations.†

Average Heights of the Barometer during the Prevalence of given Winds.

Sept. 86 observations.			Oct. 90 observations.			Nov. 89 observations.		
Winds.	No. of obs.	Barom.	Winds.	No. of obs.	Barom.	Winds.	No. of obs.	Barom.
		Lines.			*Lines.*			*Lines.*
N.	8	357·036	N.	9	356·796	N.	12	358·776
NE.	9	356·520	NE.	4	356·444	NE.	2	359·400
E.	4	356·172	E.	5	358·872	E.	3	359·868
SE.	18	355·544	SE.	10	357·264	SE.	8	359·904
S.	17	356·424	S.	29	357·984	S.	26	359·352
SW.	3	354·528	SW.	6	358·880	SW.	4	360·432
W.	5	356·796	W.	4	358·116	W.	8	358·950
NW.	22	356·376	NW.	23	355·332	NW.	26	359·256

I fear I have already too much intruded on your patience by a mention of numerical data, which I know must be sparingly introduced in a sketch like this, which is to be heard before its author can expect to find readers. I submit therefore to you the table which I have mentioned, not altogether without a hope that some of you, Gentlemen, may honour it with your attention, and in this supposition, I would be more gratified to receive information from the conclusions which you may derive from them, than to furnish inferences by my commentaries on my own investigations.

But I owe you my feeble assistance in a task which I know to be a heavy tax on time and patience. I will therefore recapitulate, as briefly as possible, the assertions of an author, whom you will probably consult before you decide on my researches and survey critically the results of my observations.

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* See this Journal, vol. iii, p. 193.

† It is important to inform the reader that these baromtrical observations are reduced to zero of the centigrade thermometer, and corrected for the capillary attraction.

Volney, notwithstanding his unkind feelings towards the country, where he had once found a refuge, and the incompetency in which his want of instruments placed him for acquiring the authority of an observer, often deserves a respectful consideration, when he generalizes data other than his own.

Count Volney asserts, that the north wind is rare in this country, except, says he, in the neighbourhood of Williamsburg. The table annexed to the present remarks warrants in some degree that assertion, but we all know that the author of the "Soil and Climate of the United States" is not correct in saying, that the *north wind is rather misty than dry, and oftener accompanied with clouds, than with a clear sky*, (vol. i, p. 176). The remarks of the author, on the southeast winds, are in accordance with our experience; but the assertion that the south winds are cooler than those from the southeast, is objectionable. However, these differences do not warrant discrediting the author's remarks on the climate of this country; and I am so much the more willing to do him that justice, as I was reluctant to admit his inferences with unlimited confidence.*

In regard to my own observations at a first inspection, or rather omitting to take into consideration the number of *observations* corresponding to every one of the 8 winds under which they are ranged in the annexed table, it would seem that in the course of either of the three months, the height of the barometer was but little affected by the change of the wind. For instance, in September the barometer was at 356 lines (omitting fractions) during 5 of the 8 winds from north to northwest, and again in November it was at 359 lines, at the same number of observations, though not in the same winds. But I would remark, that allowance must be made on account of the number of observations. Thus in September, 9 observations giving 356·52 lines, during a northwest wind, are equal in result to 3 during a south-

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* Observations on wind and weather mentioned in Dr Rush's valuable work, and made in Philadelphia during September and October 1793, show that of 6 days in September 1793, when the wind was north, but one day was cloudy. Three days of northeast wind were likewise cloudy. With southeast winds one day was fair, one cloudy, and one rainy. Two days of south winds were cloudy. Three days of southwest, one day west, and seven of northwest were fair. In October 1793, five days north, two northeast, one of east, one of southwest, four of west, six of northwest winds were fair; two days of north, one of south, and three of southwest winds were cloudy. One wind of the north and two from the southwest were accompanied with rain.

Mr Josiah Meigs states for 1820 (in Washington.)

Winds.	East	1.	Winds.	Northeast	91.
	Southeast	11.		West	108.
	North	13.		Northwest	155.
	South	43.		Southwest	177.

westerly wind. Now a great height of the barometer, resulting from a great number of observations, proves, it seems, that the effect observed in most of these cases is greater, than when demonstrated by only a few observations. The example I have quoted will illustrate my idea: 9 observations, in one wind, indicating 356·52 lines, are equal in result to 3 observations during another wind. Were then, *individually*, the former generally of smaller rate than the latter? No; but smaller quantities were compensated by greater, and these must have been more numerous than the former. Three observations may be accidentally productive of a great numerical result, but 9 observations lessen comparatively the influence of exceptions, and seem to indicate rather the rule.

I have tried various methods to bring this to a clearer view; finally, I have thought that separating the winds, according to the physiognomy and the geographical position of this country, with respect to sea and land, the investigation would at least repose upon an adequate ground. I have therefore compared the barometrical results connected with the winds from W. NW. N. and NE. with those which are conjointed with the winds S. SE. S. and SW. The latter may be called Atlantic winds, and are in the direction of the coast from the Bay of Fundy to the Strait of Pamlico, and the former are, on the contrary, continental winds. Taking then *proportionally*, their corresponding barometrical results, it appears that,

1°. *In September*, the barometer was more elevated during the prevalence of the E. SE. S. and SW. winds, than during the W. NW. N. and NE. winds.

2°. *In October*, it was, on the contrary, considerably more elevated during the W. NW. N. and NE. winds, than during the winds S. SE. S. and SW.

3°. *In November*, it was again, as during September, more elevated during E. SE. S. and SW. than in those of W. NW. N. and NE.

Taking from the observations of November the N. and NE. winds and comparing their barometrical results with those belonging to S. and SW. winds, it will be seen that the latter were considerably smaller; and taking from the same month the easterly and southeasterly winds, for a comparison with the W. and NW. winds, the former will be found accompanied with much greater barometrical heights than the latter.

*During the month of September the barometer stood gen-

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* See note, page 288.

erally the highest with winds from the north, northeast, and northwest. The average of 39 observations made during the prevalence of these winds, give us a mean height of 356·6 L. It will be seen by the annexed table, that it descended only once to 0·7481 m. (29·453 in.). Fortyseven barometrical observations during the prevalence of the other 5 winds furnish a mean height of 355·89 L.

The south and southeast winds, in which the barometer was observed 35 times, indicate but a mean height of 355·98 L. yet some of the observations, taken separately, seem to indicate a great rise of the mercury. In the column of the southern winds, the indications are twice 0·760 m. (29·922 in.) and the barometer never fell, during their prevalence, below 0·7481 m. (29·1453 in.). Calculating separately the observations, made under winds from the north, northeast, west, and northwest, we find for 44 observations, an average of 356·65 L. a result which is equal to that of 39 observations, made in the winds from north, northeast, and northwest. 42 observations in the winds from east, southeast, south, and southwest, give but 355·65 L.

Again, during the month of October, the barometer was observed 40 times in north, northeast, west, and northwest winds, and the average result of these 40 observations is 356·67 L. The average of the other 50 observations of that month, in winds from the east to southwest, is 358·2 L.

Finally, during November, 48 barometrical observations in winds from north, northeast, west, and northwest present a mean result of 359·1 L. while 41 observations in the other four winds give 359·9 L. Examining separately the observations of November, when the northwest wind prevailed, one is struck with the uncommon height of the barometer, which rose once to 0·7672 m. (30·394 in.).

Resuming these relative results of 3 months, we discover that in *October*, the barometer was higher in north, northeast, west, and northwest winds, than when the other four winds prevailed: that the contrary was observed in November and September. However, this conclusion must be qualified, on account of the number of observations corresponding to every one of these subdivisions; but the differences are so slight, that they can be neglected in calculations like these. Yet it may be seen by such an analytical examination, how quantities may be combined, in order to furnish useful results, and how useless or productive of errors they are, as long as they remain in no other regularity, than that which their chronological order gives them. According to the happy remark of Haüy, "the Arithmetic of the barometer is a surer guide, than its mere rise and fall."

The following table shows the differences from one day to another, during November, of the mean heights of the barometer.

November, 1825.

Days.	Mean height of the Barom.	Differences.
1	29·461	
2	29·727	+ 0·266
3	30·141	+ 0·414
4	30·277	+ 0·136
5	30·319	+ 0·042
6	30·188	— 0·131
7	30·053	— 0·135
8	29·874	— 0·179
9	29·834	— 0·040
10	29·915	+ 0·081
11	29·850	— 0·065
12	30·103	+ 0·253
13	30·015	— 0·088
14	29·711	— 0·304
15	29·883	+ 0·172
16	29·875	— 0·008
17	29·602	— 0·273
18	29·788	+ 0·186
19	30·083	+ 0·295
20	29·895	— 0·188
21	29·997	+ 0·102
22	29·990	— 0·007
23	30·137	+ 0·147
24	29·878	— 0·259
25	29·916	+ 0·038
26	30·003	+ 0·087
27	30·007	+ 0·004
28	30·010	+ 0·003
29	29·970	— 0·040
30	29·901	— 0·069

The rise and fall of the mercury in the barometer are generally gradual and slow. Quick changes have been observed in north and south winds, viz. from 4 h. P. M. of the 10th of September to 7 h. A. M. of the 11th the rise was 0·0073 m. or 3·404 lin. the wind being northwest and the weather fair. From the 16th

of October 4 h. P. M. to the 17th, 7 h. A. M. the change was 0·0053 m. or 2·544 lin. the wind being northwest and the weather fair. From the 1st to the 5th of November, the change was 0·0250 m. or 1 inch, the wind having blown generally from the south with a clear sky.

"The elasticity of the air of the temperate zones," observes M. de Humboldt [Tableau Physique des Régions Equinoxiales, &c. p. 90,] "varies in the same place sometimes as much as 20 lin. French measure, 25·488L.; under the tropics, where the regular winds bring constantly an air of equal temperature, from the 10th degree north to the 10th south of the equator, the elasticity does not vary, on the sea shore, more than 1·4 lin. (1·488 L.) and at 9235 feet above the level of the sea, it does not alter 0·7 lin. (0·744 L.)."

Our observations show that the variations are here more considerable than under the tropics, but less than M. de Humboldt, in his general statement, ascribes to the temperate zone. During November last, the range was 0·961 in. (11·532 L.).

The success of my endeavours to compare the mean height of the barometer at Paris with that in Washington, has been affected by the want of any detailed account of the former, except the "Annales de Chemie et Physique," for 1823, from January until May inclusive. However, taking the *mean* of the *average height* of the barometer during that interval of time at Paris, which amounts to 0·75303 m. (29·646 in.), and comparing it with the mean height of the barometer in Washington during November last, according to my observations, (0·76052 m. = 29·942 in.), it will be found that the mercury stood *here* higher than in Paris by 3·552 L.

From April 1823 until April 1824, I found the mean height 0·760 m. (20·922 in.).

M. de Humboldt [Tableau Physique des Régions Equinoxiales] has supposed in his barometrical measurements the height of that instrument on the level of the sea 0·76202 m. (30·001 in.) at + 25° of the Cent. thermometer. Reducing it to zero of the same thermometrical scale, the height would be

	m.	in.
	0·7602 =	29·931
Average of November last in Washington		29·944
Average from April 1823 to April 1824 in Washington		29·922

M. Biot says [Traité de Physique, Experimentale et Mathématique], that the mean height of the barometer is on the level of the sea 0·7629 m. (30·036 in.), the temperature being 0·8° Cent. (1·4° Fahr.). At Paris, on the level of the Seine it is

0.760 m. (29·922 in.); and according to the observations of Rohault, continued during fifteen years, it varies from 0·7669 m. (30·193 in.) to 0·7496 m. (29·513 in.) by a mean temperature of + 12° Cent. (53·6° Fahr.) [T. 1.p. 95.]

The result of my observations from 1823 to 1824 is exactly in accordance with that indicated by M. Biot for Paris, and by M. de Humboldt for the average height on the level of the ocean. M. de Humboldt says, that this latter is "the result of his observations under the tropics, as well on the coasts of the Atlantic as on those of the Pacific. Bouguer adopted only 0·76022 m. (29·922 in.) and the Spanish Geometer, Don George Juan, 29·800 in. La Condamine thought that if the mean height of the barometer, under the tropics, was not less than 29·841 in. (28 in. of Paris), it differed but little from that quantity."—"My own observations," continued M. de Humboldt, "made with barometers well purged of air by fire, and which had been compared with those of the observatory at Paris, seem to prove that the mean pressure of the air, on the level of the tropical seas, is less than in the temperate zones."

"M. Shuckburg has found the latter to be 0·76300 m. (30·040 in.) and M. Fleurian Bellevue 0·76434 m. (30·091 in.) by 12° Cent. (53·6° Fahr.). This difference of nearly two millimeters cannot be ascribed only to the difference of the mean temperature of Europe and the equatorial regions, so much the less, as in the lower districts of Peru, during the four or five months that the sun is veiled by a thick mist, the thermometer remains at 15° or 16° Cent. (59° or 60·8° Fahr.). This is a problem of as difficult solution, as the horary oscillations of the barometer under the equator."

The importance of barometrical observations needs not be urged upon you. The constitution of the atmosphere, its form, and its state in its successive regions, will only be understood, when a great number of observations, made with excellent instruments, will furnish to great mathematicians the necessary elements for this difficult discovery. Much has been done in that respect, since Pascal applied his powerful genius to this subject, and taught philosophers to determine with a little quicksilver, inclosed in a tube of glass, the various density and pressure of the atmosphere, and in consequence, the irregularities of the surface of the earth; and to navigators to escape dangers which they would not have expected when guided only by the appearances of the skies.

The barometer has caused the discovery of striking distinctive features in the countries situated near to the equator. It was

known the height of the barometer diminishes from the pole to the equatorial line.* La Condamine had already mentioned the regularity of the oscillations of the mercury in that instrument, and Halley and Cassini had investigated with sagacity the causes of this phenomenon. But M. de Humboldt gave certainty to this fact. He found regular variations at every hour of the day, not only on the coast of the Pacific, and the valleys of the Amazon, but on places elevated 12300 feet, of Paris, above the level of the sea. These variations are not influenced by the weather, nor by changes of temperature, nor the revolution of seasons, but are exclusively connected with the position of the sun. In certain places under the tropics, a time-keeper could almost be regulated by the state of the barometer. At noon the instrument stands at 338·02 lin., at 12 o'clock at night it falls to 337·88, at the 20th hour it is at 338·16.

In Europe, the climate is too variable to give such conclusive and regular results. Nevertheless the great natural philosopher Ramond has ascertained from observations continued during several years, a variation of the barometer influenced by seasons. There are two *maxima* and two *minima* resulting from these observations, the former near the summer and winter solstices, and the latter in April and September, and the mean height of each month is as the *maximum* shortly after the winter solstice, and the *minimum* in April. M. de la Place states in his "Philosophical Essay on Probabilities," that analysis proves, that these results are probable, and that they are moreover confirmed by other observations. That eminent Mathematician adds: "Time will teach if the months of the barometrical *maxima* and *minima* are coeval in the various climates. These annual and diurnal variations of the barometer are like the trade winds and monsoons, a result of the solar heat, combined with the rotary motion of the earth."

In the Memoir, which I have perhaps already too often quoted, I have mentioned a certain regularity which I have observed in the diurnal oscillations of the barometer, in the interval from the 17th of April 1823, until the 18th of April 1824, and which consisted in the diminution of the height of the mercury from 9 o'clock until 4 o'clock P. M. when the column rises again, till it reaches its *maximum*, for the space of a day, at 9 o'clock A. M. Such observations should be continued for a series of years. In experimental philosophy all causes which act with regularity may

* The average of it is a line less on the equator than between the 40th and 50th degree of latitude.

be ascertained, and their influence distinctly described by multiplied observations of these events. "And the analysis of probabilities," says M. de la Place, "fixes the probability of the existence and the intensity of the cause."

Such is the variety and the importance of the results, to which long observations may lead, that even a natural history of the atmosphere may at some future time be written; I mean a history of its state at different times. Great changes of heat are testified by the annals of antiquity. Rivers which were frozen in the times of Julius Cæsar and Augustus, the Tiber and the Rhone, flow now in the severest winters. The Black Sea is no more the hard frozen sea of Constantine Copronomus, and Germany and France are tropical regions in comparison with the degree of cold which the ancients ascribed to them. In comparison with such changes, the following may hardly seem worth noticing, to individuals who are not aware of the importance of even the smallest quantity in mathematical inquiries. According to Carlini, observations made at Milan indicate a diminution of the pressure of the atmosphere. The mean height of the barometer was

	in.	lin.
From 1764 to 1792 =	27—	9·104
From 1792 to 1801 =	27—	8·522
Difference		·582

Before I conclude, Gentlemen, this very imperfect attempt to show you rather my respect and gratitude, than to give you any information on the subjects which I have mentioned, I wish to acknowledge the satisfaction I derive from a comparison of the results of my observations, with some of the data, which were so ably presented to us in our last meeting.

My observations comprehend the interval already mentioned from the 17th of April 1823 to the 18th of April 1824. The mean heat deduced from them is 58·5° Fahr. for the whole year, 67·5° for the summer, and 47·2° for the winter. Mr Little indicates, in round numbers, 56° as the mean heat of the year. Besides that my observations did not begin with the 1st of January 1823, nor end on the 31st December of the same year, but were extended beyond the subsequent vernal equinox, a difference of 2·5° Fahr. is in the limits of the differences that may be ascribed to a difference in the method of observing, and the construction of the instruments. The mean result of Mr Little's observations for the last 3 years is 56·3°. Mr Josiah

Meigs rates the mean temperature of 1821 at 55·02°. The result I have presented it 2·7° greater than the mean heat M. de Humboldt ascribes to New York.

In his interesting dissertation on the *distribution of heat on the surface of the globe*, he observes, that the mean heat, during the month of October, can, under high northern latitudes, be considered as approaching the mean heat of the whole year. During the month of October, I found it to be 13·3° Cent. (55·9° Fahr.). The difference between the average heat of that month, and the result of all my observations for a year, was consequently but 1·4° Cent. (2·5° Fahr.). With respect to New York, M. de Humboldt rates the difference only 0·4° Cent. Our colleague states the mean temperature of October 1823 to be 54°, and the mean temperature of the whole year, 56°, difference 2°. With respect to 1824, the difference it only 1°, but during 1825 it amounted to 3°.

The *Isothermal* line of 10°, according to the ingenious system of the great German traveller, passes through this country, in 42° 45′ lat. one degree south of Dublin, half a degree north of Paris, 0° 5′ south of Prague, 1° 5′ North of Buda, and 2° 45′ north of Pekin. How astonishing must be, to persons unacquainted with this subject, such a similarity of temperature between Philadelphia, the latitude of which is 39° 56′, and Dublin, which situated in 51° 25′, and the difference of 5° Cent. (9° Fahr.) in the mean temperature of New York and Naples, though they are situated under the same latitude.

The changes of temperature are rapid in this country, and very great in comparison with the variations under the torrid zone. But it must not be forgotten that in Paris they amount to 44°, and in more northern countries of Europe to even 60°.

Gentlemen, researches in the field of science become daily more numerous, and unexpected results are the reward of indefatigable inquiry. Mayer is no more an authority. His speculations are superseded by experiments and analyses. The mean temperature of the Pole can be no more supposed to be zero, as that celebrated astronomer had stated. Arago, in discussing both hypotheses, viz. that this continent extends itself to the northern pole contiguously or by means of an Archipelago of islands, little separated from each other, and that the ocean surrounds that extremity of the axis of the earth, and availing himself of observations made from the 54° to the Melville islands, situated in 75° lat. has established, with great plausibility, that the temperature of the pole is 25° Cent.

Nay the heat of the human body has found an ingenious ob-

server. It was known that in England, the temperature of our species is 36·7° Cent. (98.1° Fahr.). Dr John Davy, in his *account of the interior* of Ceylon, has concluded from observations made with several individuals, natives of Europe and of Ceylon, that by going from a cold or temperate climate to tropical countries, the heat of man increases 1°, and in some persons 1·5° Cent.

The country to which science has been indebted for Franklin and Rumford, the country which was among the first in furnishing data for the exact determination of the figure of the earth, cannot remain indifferent to exertions for the advancement of human knowledge.

General Intelligence.

Curious Change of Colour in Oxides of Cobalt and Zinc.—It has been observed, that when a mixed solution of zinc and of cobalt is precipitated, so as to furnish a mixture of the oxides, and this mixture is well washed and dried, it forms a white powder, which, when heated in a close vessel, loses water, and becomes of a beautiful green colour, though the oxide of cobalt does not amount to above one sixth part of the whole. When well prepared, it is probable that this compound may be found to be a very useful pigment.—*Quart. Jour.* 38.

Refrigerating Salt.—If we mix 57 parts of muriate of potash, with 32 of muriate of ammonia, and 10 of nitrate of potash, a refrigerating salt will be produced. This salt, put into four parts of water, and quickly agitated, will make the thermometer descend from 20° to 5° below zero, in Reaumur's thermometer.—Vauquelin, *Journal de Pharmacie.*

Iodine in Mineral Waters.—Iodine was first discovered in mineral waters by M. Angelini, who found it in the salt water of Voghera, and in the water of Sales in the Voguerais. M. Cantu, Professor of Chemistry at Turin, surprised at the wonderful effects of the sulphurous water of Castel Nova d'Asti, in the treatment of goitres, and other glandular maladies, examined it chemically, and found it more rich in iodine than any other.—*Mem. de Torino*, tom. xxix. p. 221.

Remarkable Hailstones with pyritic Nuclei.—The following very curious account of these hailstones is given in a letter from

Professor John of Berlin, to Baron Ferussac. It forms also part of a letter from Dr Eversmann, known by his travels in Asia, dated 16th September, from Orenburg, where he has landed property.

"Some days before our arrival at Sterlitamak, (more than 100 versts from Orenburg,) there arose a storm in which very remarkable hail fell. *The hailstones, which were tolerably large, contained a stoney and crystallized nucleus.* Thirty of them have been sent to our Governor, and I have received two specimens." They are of a brown colour like the auriferous pyrites of Beresowsky, in Siberia. Their surface is shrivelled and shining. The crystal forms a flattened octoëdron, whose edges are salient. The two diagonals of the base are five lines by four, and the distance of the summits is two lines. Sometimes the four angles of the base are truncated. *It seems that the constituent parts of these crystals are sulphur and metals.* No analysis of them has yet been made, but I shall perhaps have occasion to do this.—See *Bull. des. Sc. Phys.* Feb. 1825, p. 117.

Distance to which minutely-divided Matter may be carried by the Wind.—On the morning of the 19th of January last, Mr Forbes, on board the Clyde East Indiaman, bound to London, in lat. 10 deg. 40 min. N. and long. 27 deg. 41 min. W. and about 600 miles from the coast of Africa, was surprised to find the sails covered with a brownish sand, the particles of which, being examined by a microscope, appeared extremely minute. At 2 P. M. the same day, some of the sails being unbent, clouds of dust escaped from them, on their flapping against the masts. During the night, the wind had blowed fresh NE. by E. and the nearest land to windward was that of the African coast lying between Cape de Verd and the river Gambia. May not the seeds of many plants, found in remote and newly-formed islands, have been thus conveyed?—[*New Monthly Mag.*]

Variations in the Composition of the Atmosphere.—Mr Dalton states that he has found the oxygen in the atmosphere vary from 20·7 per cent. to 21·15 per cent. The latter was the case on the 8th of January last, the barometer being 30·9, wind NE. and very moderate, after three days of calm and gentle frost. The general state of the atmosphere yields only 20·7 or 20·8 per cent. of oxygen.—[*Ann. Phil.*]

Literary Property in France.—The king of France has appointed a commission to prepare a law for the protection of literary property. The members are, the Marquis of Lally—

Tollendal, Viscount Lainé, and Count Portalis, peers of France; Royer Collard, the Count de Montbron, and Pardessus, deputies; Bellart and de Vatimesnil, privy counsellors; Delaville and Villemain, magistrates; Auger, Raynouard, Andrieux, Parseval-Grandmaison, Picard, Al. Duval, Michaud, Dacier, Baron Cuvier, Baron Fourrier, and Quatremère de Quincy, members of the four Academies; and Baron Taylor, royal commissioner at the Théâtre-Français. To these are to be added Messrs Lemercier, Etienne, Moreau, and Champein; who, on the royal requisition, have been elected by the dramatic authors as delegates, to take a share in that part of the discussions of the commission which will relate to the interests of dramatic literature. At the first meeting of the commission it was unanimously resolved to invite the attendance of M. Talma, in consequence of the close connexion between the interests of dramatic authors, and those of actors. A long *projet* of a law has been submitted to the consideration of the commission, divided into twenty distinct heads; which, after they have been thoroughly discussed, will be formed into the four following chapters:—On the property of literary works in general; on the property of dramatic works; on the property of works of art; on the property of musical compositions.—[*New Monthly Mag.*]

Patents.—Two patents have recently been obtained, which promise very brilliant advantages to their respective proprietors, and a decided benefit to the public. The first is a French discovery of an extremely hard and beautiful cement or stucco, manufactured from lime and some other ingredients, by which every kind of marble may be imitated. It is as hard as marble, and resembles it closely. Such is its closeness of texture, that it will admit of a brick being struck against it till reduced to pieces, without its effecting the least scratch or indenture. Another admirable advantage is, that it admits of fresco painting, which will endure constant washing and cleaning without suffering any injury. Oil painting can be laid over the cement as well as water colours. Neither wet, heat, cold, nor frost, make any impression, nor do they, to all appearance, in the least disturb its particles; it will not flake off. The second patent is for ornaments of various kinds in *papier mache*. The specimens already produced are equal to the finest carvings; at present it is intended to supersede plaster ornaments for gilded frames, either of pictures or looking-glasses, and for embossed ornaments in panneling splendid rooms. If it be good for frames, it must also be good for friezes and cornices of rooms, halls, staircases, &c. Its

peculiar advantages are, that it is extremely hard, very light, and cheap.—[*New Monthly Mag.*]

Mr Pond's Method of determining the Direction of the Meridian.—This method consists in placing two very distinct and well defined objects nearly at equal distances on each side of the north meridian line, and at a distance from each other nearly equal to twice the greatest elongation of the pole star. These objects are placed by means of a telescope, fixed like a transit on a horizontal axis, and pointed to the pole star, at its greatest elongation, and then at its reflected image, the central wire, which must necessarily describe a strictly vertical circle, will pass over some terrestrial object, which, if well defined, will serve for the mark; but if not, a mark must be set up so as to be bisected by the wire. The same being on the other side of the north meridian, the distances of the marks from the central wire must be accurately measured with a micrometer. The horizontal angles between the marks being measured by a theodolite, a meridian mark is to be erected exactly between them.—[*Brewster's Journal.*]

Remarkable Effect on the Cambridge Transit Instrument.—In adjusting the Transit Instrument recently put in the New Observatory at Cambridge, Mr Woodhouse observed, that the line of collimation deviated occasionally to the east or west of the meridian mark without any visible cause. He at last found that this effect arose from the approach of the assistant's body to the lateral braces used to steady the instrument in an invariable position at right angles to its axis. The expansion of the brace nearest him thrust the axis of the telescope aside. Mr Woodhouse proved that this was the cause of the deviation, by producing the same effect with hot cloths wrapped round the alternate braces, and he, therefore, provided a proper apparatus to protect the braces from the solar heat, during the approach of the sun to the meridian.—[*Brewster's Journal.*]

Committee for the Improvement of Glass for Optical Purposes.—We understand that the Royal Society, of London, and the Board of Longitude, are taking effectual measures for the improvement of Glass for optical purposes. A regular glasshouse has been built for this purpose, and a series of experiments will be immediately begun, under the direction of a committee of members of the Royal Society, from whose talents and diligence we anticipate the happiest results.—[*Brewster's Journal.*]

Electricity of Flame.—M. Pouillet has shown that, in a vertical flame formed by the combination of hydrogen with oxygen, the visible part of the flame, and the part without it, to the distance of a centimetre, is positively electrified, while, in the interior of the flame, resinous electricity alone is found.—*Ferussac's Bulletin.*]

Huygens' Mathematical Manuscripts.—A part of these manuscripts contain his correspondence with the philosophers of his time. It is to be regretted that he preserved only those which were the most interesting to him. Among them are his correspondence with Leibnitz and l'Hôpital, entire. Another part of these manuscripts consisting of several volumes written by the hand of Huygens, contain essays on several of his theories. The manuscripts of his published works have likewise been found. Joined to his dissertation on the percussion of bodies, are several pieces, to show, as it appears, the periods when his researches on this subject were made.

M. Uylenbrock, lecturer in the University of Leyden, proposes to publish the correspondence with Leibnitz and l'Hôpital, which is upon the differential calculus; in which Leibnitz explains the use of the calculus, to Huygens, and l'Hôpital solves several problems that the latter had proposed to him. The editor confining himself to the pieces of great interest, believes that he shall be able to reduce the work to 3 vols. 4to, and perhaps even to one.—[*Ferussac's Bulletin.*]

Light produced during Crystallization.—Dobereiner states, that a splendid instance of light produced during crystallization was observed by M. Buchner, of Magonza, during the sublimation of benzoic acid, previously mixed with pulverized carbon. The sublimation was carried on in a tall glass cylinder upon a stove, and when it had well begun there appeared an uninterrupted succession of sparks, continuing for half an hour, and which ceased only when the cylinder was removed from the stove.

Dobereiner states, that he has reason to believe those salts, containing no water of crystallization, are especially powerful in producing light during their crystallization.—*Gior. di Fisica*, vii, 470.

Test of the Presence of Muriatic or Nitric Acid, or Salts of these Acids.—For Muriatic Acid, or Muriates. Put pure nitric acid into a porcelain capsule, and throw to the bottom of it a very minute quantity of finely-divided gold, precipitated from its

solution by sulphate of iron; put into the acid a minute fragment of any muriate, by degrees the gold will be surrounded by a light yellow tint, a certain sign that it is attacked. A fiftieth of a grain of a muriate may be detected in this manner.

For Nitric Acid or Nitrates. Operating in an inverse manner, the presence of a nitrate in a soluble salt may be ascertained. The gold should be put into muriatic acid as pure and as colourless as possible, and then a fragment of the salt, supposed to contain a nitrate, added; sometimes this experiment requires many hours for its completion, if there be but a small quantity of nitrate present.

This variation of Dr Wollaston's test, may at times be usefully employed.—[*Ann. de Chimic.* xxviii, 36.]

Meteor.—A brilliant meteor was observed on the evening of March 31st. about 8 o'clock, in the SSW.; its elevation when first observed being apparently 40°. A strong stream of light was thrown out in the direction from which it descended. Before it descended, the light disappeared entirely. From the appearances, it is probable that an aerolite (or meteoric stone) fell somewhere in this vicinity—Cambridge. It appears by an account from New Haven, that the same meteor was seen at that place, passing in a westerly direction. Its passage was accompanied with an explosion.—[W].

Celestial Phenomena for the months of April and May 1826, *computed for Cambridge, in lat.* 42° 23′ 28″ *N. and long.* 71° 7′ 25″ *W.* Mean time. Communicated by R. T. Paine, Esq.

April 1826.

D.	H.	M.	S.		
3	9	17	28	P. M.	☌ ☽ and Situla, in the ecliptic.
4					Greatest elong. of ☿ east of ☉.
5	2	57	40	A. M.	Em. ♃ 1st. Sat.
6	8	37	07	P. M.	Em. ♃ 2d. Sat.
"	9	26	14	"	Em. ♃ 1st. Sat.
12	5	5	16	"	✱ ♂ ☽ and ξ ♉.
13	4	43	19	"	✱ ♂ ☽ and ν ♊.
"	11	14	16	"	Em. ♃ 2d. Sat,
"	11	20	35	"	Em, ♃ 1st. Sat.
14	7	18		A. M.	☐ of ♅ W. of ☉.
16	6	28	38	P. M.	☌ ☽ and ♐ ♌, in the ecliptic, at nearest ap. ✱ 3′ ap. south of ☽.

D.	H.	M.	S.		
18	7	55	33	"	✱ ♂ ☽ and 55 ☊ in the ecliptic.
"	7	32	22	"	Em. of 55 ☊ 1′, 3 s. of ☽'s centre.
20	10	57	32	A. M.	☉ enters ♉.
21	1	15	01	"	Em. ♅ 1st. Sat.
"	1	51	31	"	Em. ♅ 2d. Sat.
22	9	36	59	P. M.	Em. ♅ 3d. Sat.
23	5	25	27	A. M.	♂ ☽ and ι ♎ in the ecliptic.
24	9	28		"	Inf. ♂ of ☿.
29	9	38	09	P. M.	Em. ♅ 1st Sat.
"	10	20	21	"	Em. ♅ 2d. Sat.
30	1	35	29	A. M.	Em. ♅ 2d. Sat.

May 1826.

D.	H.	M.	S.		
1	2	59	08	A. M.	♂ ☽ and situla, a very near approach.
2	5	27	01	"	♂ ☽ and ϰ ♓, a near approach.
5	2	13		"	☌ of ♂.
6	11	32	43	P. M.	Em. ♅ 1st Sat.
8	8	24	12	"	Em. ♅ 2d. Sat.
9	10	57	27	"	♂ ☽ and ζ ♉.
10	10	39	58	"	♂ ☽ and ν ♊.
13	1	51	13	P. M.	✱ ♂ ☽ and A ♋.
"	7	55	35	"	♂ ☽ and 1 α ♎ in the ecliptic.
"	8	0	02	"	Immersion 1·5′ ☽ north of ☽'s
"	9	7	49	"	Emersion 6·9 ☽ centre.
"	8	49	47	"	☌ ☽ and Acubens.
14	1	27	20	A. M.	Em. ♅ 1st. Sat.
15	11	01	28	P. M.	Em. ♅ 2d. Sat.
16	8	42	31	"	☌ ☽ and e ♌ in the ecliptic.
"	7	49	37	"	Immersion 3·2′ south ☽ of ☽'s
"	9	5	59	"	Emersion 1·1′ north ☽ centre.
21					☽ tot. ecl. invisible.
"	11	19	3	A. M.	☉ enters ♊.
22	2				Great. elong. of ☿ W. of ☉.
"	9	50	39	P. M.	Em. ♅ 1st. Sat.
27	4	06		A. M.	□ of ♅ E. of ☉
29	11	45	19	P. M.	Em. ♅ 1st Sat.

(To be continued.)

ERRATA.

In the notice of the comet, page 101, for 51° 32′ read 56° 32′. In the celestial phenomena, page 102, Nov. 26th. for 5 h. 41 m. read 5 h. 47 m.; and in the second line of the note at bottom of same page, *dele* "; " after "sunset."

METEOROLOGICAL OBSERVATION

Lat. 38° 58′, *Long.* 77° 2′, *for the months of*

BY J. DE WA

September.		Winds and corresponding Heights of the Barometer.								
Days.	H'rs.	North.	N. E.	East.	S. E.	South.	S. W.	West.	N. W.	State of the Weather.
	7	0.7535	..	..	..	..	..	..	..	clear, strong wind.
1	12	0.7544	..	..	..	..	..	..	..	id. id.
	4	0.7543	..	..	..	..	..	..	..	id. id.
	7	..	0.7566	..	..	..	..	..	..	smoky, calm.
2	12	..	0.7567	..	..	..	..	..	..	do. do.
	4	..	0.7553	..	..	..	..	..	..	do. do.
	..	..								
3	..	..								no observation.
	..	..								
	7	..	0.7586	..	..	..	..	..	..	fair, brisk wind.
4	12	0.7576	..	..	..	..	..	..	..	id.
	4	0.7574	..	..	..	..	..	..	..	clear and calm.
	7	0.7578	..	..	..	..	..	..	..	fair and calm.
5	9	0.7585	..	..	..	..	..	..	..	id.
	12	..	..	..	0.7566	..	..	..	..	very cloudy.
	7	..	..	..	..	0.7565	..	..	..	fair, calm.
6	12	..	..	..	0.7563	..	..	..	..	id. light wind.
	4	..	..	..	0.7547	..	..	..	..	cloudy, do.
	7	..	..	..	0.7542	..	..	..	..	do. light wind.
7	12	..	..	..	..	0.7517	..	..	..	do. brisk do.
	4	..	..	..	..	0.7499	..	..	..	very cloudy, id.
	7	..	..	..	..	..	..	..	0.7523	light rain, calm.
8	12	..	..	..	..	0.7527	..	..	..	rain, do.
	4	..	..	..	..	..	..	..	..	no observation.
	7	..	..	..	..	0.7521	..	..	..	very fine, light wind.
9	12	..	..	..	..	..	..	0.7512	..	id. id.
	4	..	..	..	..	..	..	..	0.7519	cloudy, strong wind.
	7	..	..	..	..	..	..	..	0.7547	fair, fine breeze.
10	12	..	..	..	..	..	..	..	0.7547	id. id.
	4	..	..	..	..	..	..	..	0.7537	id. id.
	7	..	..	..	..	..	..	0.7610	..	fair, calm.
11	12	..	..	0.7610	..	..	..	..	..	do. do.
	4	..	..	..	0.7602	..	..	..	..	do. light wind.
	7	..	..	..	..	0.7607	..	..	..	cloudy, light wind.
12	12	..	..	..	..	0.7601	..	..	..	less cloudy id.
	4	..	..	..	..	0.7581	..	..	..	light clouds id.
	7	..	..	..	0.7573	..	..	..	..	very hot, sultry, id.
13	12	..	..	..	0.7567	..	..	..	..	id. cloudy, id.
	4	..	..	..	..	0.7541	..	..	..	id. id. id.
	7	..	..	..	..	..	0.7541	..	..	very close, smoky, calm.
14	12	..	..	..	..	..	..	..	0.7537	clear, calm.
	4	..	..	..	0.7510	..	..	..	..	smoky, calm.
	7	..	..	..	..	..	..	..	0.7545	fine weather, calm.
15	12	..	..	..	..	0.7542	..	..	..	id. id.
	4	..	..	..	0.7524	..	..	..	..	id. id.
	7	..	..	0.7512	..	..	..	..	..	very cloudy, calm.
16	12	..	..	..	0.7500	..	..	..	..	rain, calm.
	4	..	..	..	0.7492	..	..	..	..	id. id.
	7	..	..	..	..	..	..	..	0.7481	fair, id.
17	12	..	..	..	..	..	0.7483	..	..	cloudy, calm.
	4	..	..	..	..	..	0.7488	..	..	id. id.
	7	..	..	..	..	..	..	..	0.7548	cloudy, light wind.
18	12	..	..	..	..	..	..	..	0.7548	id. id.
	4	..	..	..	..	..	..	..	0.7550	id. id.
	7	..	..	..	..	..	..	..	0.7569	very fine, calm
19	12	..	..	..	..	..	..	..	0.7569	id. id.
	4	..	..	..	..	0.7559	..	..	..	id. id.
	7	..	..	..	..	0.7557	..	..	..	id. id.
20	12	..	..	..	0.7537	..	..	..	..	id. light wind.
	4	..	..	0.7519	..	..	..	..	..	id. id.
	7	..	0.7520	..	..	..	..	..	..	fair, calm.
21	12	..	..	..	..	0.7519	..	..	..	cloudy, id.
	4	..	..	..	0.7503	..	..	..	..	fair, id.
	7	..	..	0.7517	..	..	..	..	..	foggy, light wind.
22	12	..	..	..	0.7519	..	..	..	..	cloudy, id.
	4	..	..	..	..	0.7503	..	..	..	clear, id.
	7	..	0.7513	..	..	..	..	..	..	close and foggy, light winds.
23	12	..	..	..	..	0.7506	..	..	..	sultry and cloudy, id.
	4	..	..	..	..	0.7481	..	..	..	rain, calm.
	7	..	..	..	..	..	..	0.7471	..	fair, light wind.
24	12	..	..	..	..	..	..	..	0.7486	cloudy, brisk wind.
	4	..	..	..	..	..	..	..	0.7492	id. id.
	7	..	0.7560	..	..	..	..	..	..	id. id.
25	12	..	..	..	0.7562	..	..	..	..	id. id.
	4	..	..	..	..	..	..	0.7554	..	id. id.
	7	..	..	..	..	..	..	..	0.7544	very cloudy, calm.
26	12	..	0.7529	..	..	..	..	..	..	rain, light wind.
	4	..	0.7519	..	..	..	..	..	..	light rain, id.
	7	0.7512	..	..	..	..	..	..	..	rainy, light wind.
27	12	..	..	..	..	..	..	..	0.7521	cloudy, strong wind.
	4	..	..	..	..	..	..	..	0.7512	clear, strong wind.
	7	..	..	..	..	..	..	..	0 7585	fair, brisk wind.
28	12	..	..	..	..	..	..	..	0.7581	cloudy, id.
	4	..	..	..	..	..	..	..	0.7589	id. strong wind.
	7	..	..	..	..	..	..	..	0.7623	fine weather and calm.
29	12	..	..	..	..	0.7623	..	..	..	clouds and light wind.
	4	..	..	..	0.7607	..	..	..	..	fair, id.
	7	..	..	..	..	..	..	0.7606	..	clear and calm.
30	12	..	..	..	0.7588	..	..	..	..	id. light wind.
	4	..	..	..	0.7573	..	..	..	..	id. id.
		0.7557	0.7546	0.7539	0.7548	0.7544	0.7507	0.7552	0.7543	

October.		Winds and corr				
Days.	H'rs.	North.	N. E.	East.	S. E.	South
	7	..	..	..	..	..
1	12	..	..	..	..	..
	4	..	..	..	..	..
	7	..	..	..	..	..
2	12	..	..	..	..	0.756
	4	..	..	..	..	0.756
	7	..	..	..	..	..
3	12	..	..	..	0.7548	..
	4	..	..	..	..	0.752
	7	..	..	..	..	0.749
4	12	..	..	..	..	0.767
	4	..	0.7469	..	..	..
	7	..	..	..	..	..
5	12	0.7518	..	..	..	..
	5	0.7526	..	..	..	..
	7	..	..	..	..	..
6	12	0.7556	..	..	..	..
	4	..	..	..	0.7555	..
	7	..	..	..	..	0.756
7	12	..	..	..	..	0.75
	4	0.7544	..	..	..	..
	7	..	..	..	..	..
8	12	0.7569	..	..	..	..
	4	..	..	0.7569	..	..
	7	..	..	0.7619	..	..
9	12	..	..	0.7619	..	..
	4	..	..	0.7606	..	..
	7	..	..	..	0.7585	..
10	12	..	..	..	0.7570	..
	4	..	..	..	0.7547	..
	7	..	..	..	..	..
11	12	..	..	..	..	..
	4	..	..	..	..	..
	7	0.7601	..	..	..	..
12	12	..	0.7601	..	..	..
	4	..	0.7601	..	..	..
	7	..	..	..	0.7573	..
13	12	..	..	..	0.7573	..
	4	..	..	..	0.7574	..
	7	..	..	0.7568	..	..
14	12	..	..	..	..	0.75
	4	..	0.7540	..	..	..
	7	..	..	..	..	0.75
15	12	..	..	..	..	0.75
	4	..	..	..	..	0.74
	7	..	..	..	..	..
16	12	..	..	..	..	..
	4	..	..	..	..	..
	7	..	..	..	..	..
17	12	0.7524	..	..	..	..
	4	..	..	..	..	..
	7	..	..	..	..	..
18	12	..	..	..	..	..
	4	..	..	..	..	..
	7	..	..	..	..	..
19	12	.	..	..	..	..
	4	..	..	..	..	..
	7	..	..	..	..	..
20	12	..	..	..	..	0.75
	4	..	..	..	..	..
	7	..	..	..	..	..
21	12	..	..	..	..	..
	4	..	..	..	..	..
	8	..	..	..	..	..
22	1	..	..	..	..	0.76
	4	..	..	..	..	0.76
	8	..	..	..	..	0.76
23	1	..	..	..	..	0.75
	4	..	..	..	..	0.75
	8	..	..	..	..	..
24	12	..	..	..	..	..
	5	0.7543	..	..	..	..
	7	..	..	..	..	0.75
25	1	..	..	..	0.7591	..
	4	..	..	..	..	0.75
	8	..	..	..	..	0.76
26	12	..	..	..	..	0.75
	4	..	..	..	..	0.75
	8	..	..	..	..	0.75
27	12	..	..	..	..	0.74
	4	..	..	..	..	0.76
	8	..	..	..	..	..
28	12	..	..	..	..	..
	4	0.7588	..	..	..	..
	8	..	..	..	..	..
29	12	..	..	..	..	..
	4	..	..	..	..	0.76
	8	..	..	..	..	..
30	1	..	..	..	..	..
	4	..	..	..	..	0.75
	8	..	..	..	..	0.75
31	1	..	..	..	0.7503	..
	4	..	..	..	..	0.74
		0.7552	0.7563	0.7596	0.7562	0.75

ONS MADE AT WASHINGTON, D. C.

ns of September, October, and November, 1825.

WALLENSTEIN.

corresponding Heights of the Barometer.					November.		Winds and corresponding Heights of the Barometer.								
South.	S. W.	West.	N. W.	State of the Weather.	Days.	H'rs.	North.	N. E.	East.	S. E.	South.	S. W.	West.	N. W.	State of the Weather.
..	..	..	..			7	..	..	..	..	..	..	..	0.7500	clear and calm.
..	..	..	..	no observation.	1	12	..	..	..	..	0.7490	..	..	..	id. light wind.
..	..	..	..	id.		4	..	..	..	..	0.7459	..	..	..	cloudy, id.
..	0.7573	..	..	fair, calm.		8	..	..	..	..	0.7539	..	..	..	fair, calm.
0.7564	..	..	..	id. id.	2	12	..	..	..	..	..	..	0.7550	..	clear, strong wind.
0.7561	..	..	..	id. id.		4	..	..	..	..	..	..	0.7563	..	id. id.
..	..	..	0.7568	cloudy and calm.		8	..	..	..	..	..	..	..	0.7655	fine, calm.
..	..	..	..	id. wind.	3	12	..	..	..	..	0.7665	..	..	..	id. id.
0.7528	..	..	..	rain, wind.		4	..	..	..	..	0.7652	..	..	..	id. id.
0.7496	..	..	..	overcast, calm.		8	..	..	..	..	..	0.7686	..	..	id. id.
0.7679	..	..	..	id. light wind.	4	12	..	..	..	..	0.7692	..	..	..	id. id.
..	..	..	..	id. calm.		4	..	..	..	..	0.7692	..	..	..	id. id.
..	..	..	0.7607	fair, light wind.		8	..	..	..	..	..	..	.	0.7709	hazy, id.
..	..	..	..	id. id.	5	12	..	..	..	..	0.7703	..	..	..	clear, id.
..	..	..	..	id. calm.		4	..	..	..	0.7691	..	..	..	..	id. id.
..	..	..	0.7559	very fine, calm.		8	..	..	..	..	0.7684	..	..	..	light fog, id.
..	..	..	..	id. light wind.	6	12	..	..	..	..	0.7666	..	..	..	fair, id.
..	..	..	..	id. calm.		4	..	..	..	..	0.7653	..	..	..	id. id.
0.7564	..	..	..	foggy, id.		8	..	..	..	..	..	..	..	0.7652	light fog, id.
0.7560	..	..	..	cloudy, id.	7	12	..	..	..	0.7629	..	..	..	..	fair, id.
..	..	..	..	clear, id.		4	0.7619	..	..	..	..	..	..	..	id. id.
..	..	..	0.7564	fair, windy.		8	..	..	..	..	..	..	..	0.7607	light fog, id.
..	..	..	..	id. light wind.	8	12	..	..	..	..	0.7590	..	..	..	clear, id.
..	..	..	..	id. brisk wind.		4	..	..	..	..	0.7567	..	..	..	cloudy, id.
..	..	..	..	thick fog, light breeze.		8	0.7582	..	..	..	..	..	..	..	overcast, strong wind.
..	..	..	..	id. id.	9	12	0.7573	..	..	..	..	..	..	..	rain, id.
..	..	..	..	id. id.		4	0.7578	..	..	..	..	..	..	..	cloudy, id.
..	..	..	..	hazy, calm.		8	..	..	..	..	..	..	..	0.7599	fair, calm.
..	..	..	..	id. fresh breeze.	10	12	..	..	..	..	..	..	0.7600	..	id. id.
..	..	..	..	clear, light wind.		4	0.7596	..	..	..	..	..	..	..	id. id.
..	0.7530	..	..	in the morning a heavy shower.		8	..	..	..	..	..	0 7591	..	..	cloudy, id.
..	..	..	0.7549	light clouds calm.	11	12	..	..	..	..	0.7577	..	..	..	id. id.
..	..	..	0.7551	id. id. id.		4	0.7580	..	..	..	..	..	..	..	id. id.
..	..	..	..	light clouds brisk wind.		8	..	..	..	..	..	..	0.7665	..	fair, id.
..	..	..	..	cloudy, light wind.	12	12	..	..	..	..	..	..	0.7641	..	id. light wind.
..	..	..	..	id. id.		4	0.7642	..	..	..	..	..	..	..	id. calm.
..	..	..	..	id. calm.		8	..	..	..	..	0.7650	..	..	..	id. id.
..	..	..	..	id. light wind.	13	12	..	..	..	..	0.7620	..	..	..	id. brisk wind.
..	..	..	..	id. calm.		4	..	..	..	0.7601	..	..	..	..	id. id.
..	..	..	..	id. id.		8	0.7530	..	..	..	..	..	..	..	rain, strong wind.
0.7541	..	..	..	clear, id.	14	12	..	..	..	..	..	..	..	0.7550	cloudy, wind abating.
..	..	..	..	id. light wind.		4	..	..	..	..	..	..	..	0.7564	fair, calm.
0.7537	..	..	..	cloudy, calm.		8	..	..	..	..	0.7592	..	..	..	cloudy, light wind.
0.7523	..	..	..	id. id.	15	12	..	..	..	..	0.7690	..	..	..	id. id.
0.7494	..	..	..	clear, light wind.		4	..	..	..	0.7589	..	..	..	..	id. id.
..	..	..	0.7490	id. strong wind.		8	0.7588	..	..	..	..	..	..	..	clear, calm.
..	..	..	0.7487	id. id.	16	12	..	..	..	..	..	..	0.7587	.	id. light wind.
..	..	..	0.7486	id. id.		4	..	..	..	..	..	..	..	0.7590	id. id.
..	..	..	0.7539	id. less wind.		8	..	..	..	..	0.7549	..	..	..	cloudy, calm.
..	..	..	..	cloudy, brisk do.	17	12	..	..	..	..	0.7569	..	..	..	id. id.
..	..	..	0.7523	smoky, id.		4	..	..	..	..	0.7497	..	..	..	id. id.
..	..	..	0 7573	cloudy, strong wind.		8	..	..	..	..	..	..	..	0.7552	clear, violent wind.
..	..	..	0.7570	id. id.	18	12	..	..	..	..	..	..	..	0.7561	id. id.
..	..	..	0.7577	id. id.		4	..	..	..	..	..	..	..	0.7585	id. id.
..	0.7596	..	..	clear, calm.		8	..	..	..	..	..	..	..	0.7650	fair, calm.
..	..	..	0.7602	id. brisk wind.	19	12	..	..	..	..	..	0.7644	..	..	id. id.
..	..	0.7588	..	id. id.		4	..	..	..	..	0.7629	..	..	..	id. id.
..	..	0.7592	..	cloudy, calm.		8	..	..	..	..	0.7601	..	..	..	cloudy, light wind.
0.7599	..	.	..	id. id.	20	12	..	..	..	..	..	..	0.7597	..	fair, calm.
..	0.7577	..	.	clear, id.		4	..	..	..	0.7588	..	..	..	..	id. id.
..	..	..	0.7585	id. id.		8	0.7624	..	..	..	..	..	..	..	hazy, id.
..	..	..	0.7600	id. strong wind.	21	12	..	..	..	0.7620	..	..	..	..	cloudy, id.
..	..	..	0.7618	id. id.		4	..	..	..	..	0.7613	..	..	..	id. id.
..	..	..	0.7693	fair, calm.		8	..	..	..	..	..	..	..	0 7617	foggy, calm.
0.7688	..	..	..	id. id.	22	12	..	..	..	..	..	..	..	0.7615	id. brisk wind.
0.7672	..	..	..	id. id.		4	..	..	..	..	..	..	..	0.7620	light fog, calm.
0.7633	..	..	..	id. id.		8	..	..	..	..	..	..	..	0.7672	fair, calm.
0.7587	..	..	..	id. light wind.	23	12	..	..	..	..	0.7658	..	..	..	id. id.
0.7588	..	..	..	id. id.		4	..	..	..	..	0.7636	..	..	..	id. id.
..	..	0.7561	..	id. calm.		9	..	..	..	..	..	0.7597	..	..	cloudy, id.
..	..	..	0.7561	cloudy, light wind.	24	12	..	..	..	..	..	..	..	0.7586	id. id.
..	..	..	..	id. calm.		4	..	..	..	..	..	..	..	0.7584	id. brisk wind.
0.7592	..	..	..	id. id.		9	..	..	..	..	..	..	..	0.7603	fair and calm.
..	..	..	..	clear, id.	25	"	..	..	..	..	..	..	..	..	no observation.
0.7583	..	..	..	cloudy, light wind.		4	..	..	..	..	..	..	..	0.7594	fair, calm.
0.7601	..	..	..	id. calm.		9	..	..	..	..	..	..	..	0.7623	id. id.
0.7588	..	..	..	id. strong wind.	26	12	..	..	..	..	..	..	..	0.7621	id. id.
0.7569	..	..	..	id. id.		4	..	..	0.7618	..	..	..	..	..	id. id.
0.7529	..	..	..	light rain, wind.		8	..	..	..	..	..	..	..	0.7630	fair, calm, hoar frost.
0.7496	..	..	..	id. id.	27	12	0.7624	..	..	..	..	..	..	..	fair, calm.
0.7670	..	..	..	id. id.		4	..	..	0.7611	..	..	..	..	..	id. id.
..	..	..	0.7582	clouds, very strong wind.		8	..	..	..	..	..	..	..	0.7625	id. id.
..	..	..	0.7596	clear, id.	28	12	..	..	..	..	..	..	..	0.7620	id. id.
..	..	..	..	id. id.		4	..	..	0.7622	..	..	..	..	..	id. id.
..	0.7638	..	..	fair, calm.		8	..	0.7611	..	..	..	..	..	..	id. id.
..	0.7638	..	..	id. id.	29	12	..	..	..	0.7621	..	..	..	..	id. id.
0.7615	..	..	..	clouds, id.		4	..	..	..	0.7606	..	..	..	..	id. id.
..	..	..	0.7603	fair, id.		8	..	0.7603	..	..	..	..	..	..	cloudy, calm.
..	..	0.7573	..	clouds, id.	30	12	0.7596	..	..	..	..	..	..	..	id. id.
0.7575	..	..	..	clear, id.		4	..	..	..	..	..	..	0.7588	..	id. id.
0.7540	..	..	..	id. id.											
..	..	..	..	id. id.			0.7594	0.7607	0.7618	0.7618	0.7606	0.7629	0.7598	0.7604	
0.7481	..	..	..	clouds, light winds.											
0.7577	0.7592	0.7580	0.7521												

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